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CONFLICT AND CONFORMITY:

A PROBABILITY MODEL AND ITS APPLICATION

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To the memory of Professor Samuel Andrew Stouffer

FOREWORD

There has long been a need in science and engineering for systematic publication of research studies larger in scope than a journal article but less ambitious than a finished book. Much valuable work of this kind is now published only in a semiprivate way, perhaps as a laboratory report, and so may not find its proper place in the literature of the field. The present contribution is the eighteenth of the M. I. T. Press Research Monographs, which we hope will make selected timely and important research studies readily accessible to libraries and to the independent worker.

J. A. Stratton

PREFACE

At every stage, this research has benefited from the advice of many individuals and the support of several institutions. The author wishes to express his appreciation for all the aid he has received. He is especially grateful to Professor Robert R. Bush for his patient advice and instruction, and to Professor Solomon Asch for his many helpful suggestions and for permission to use his data. It is fair to say that the prior work of these two investigators made the present study possible.

The author also wishes to acknowledge the assistance he received at each stage of the project. The initial studies were conducted while the author held a Research Training Fellowship given by the Social Science Research Council. This fellowship, together with attendance at the S. S. R. C. Summer Institute in Mathematics for Social Scientists, enabled the author to formulate his model and apply it to the Moderate and Extreme conditions. In the course of this phase of the study, the suggestions with respect to design of the experiment and analysis of the data by Professors Robert F. Bales, Sanford Dornbusch, and Frederick Mosteller were invaluable.

The Moderate and Extreme conditions were conducted with the cooperation of Dr. A. Paul Hare and his class in Social Psychology. The students in this course served as confederates and by their performance contributed greatly to the project.

Experiment 3 and the initial phases of our computer research took place while the author was on the staff of the Computation Center at M. I. T. and the Department of Social Relations at Harvard. Professors Dean N. Arden and Philip M. Morse were instrumental to our understanding and appreciation of the computer's role in this investigation. Mr. Thomas N. Tentler and Miss Joan Levin, who helped conduct and analyze the experiment, deserve sincere thanks, and we are particularly indebted to the Computation Center and the Laboratory of Social Relations at Harvard for providing the facilities and the funds for this stage of the project.

The computer analyses were continued with the aid of the facilities of the Computing Installation of Standard Oil of California and the support of IBM. We are especially grateful to Dr. Roger Simons of IBM for arranging access to these facilities and for working with the author long into the night to obtain the computer output.

The final phases of the project and the revisions of the manuscript were completed under grant number G-9030 from the National Science Foundation. In this last phase, Professor J. Laurie Snell was extremely helpful in clarifying some aspects of our estimation problems. Mr. Thomas Mayer worked through much tedious algebra to check our derivations.

The author benefited greatly from the critical reading of the manuscript and from discussions with his colleagues. He particularly wants to thank Professors Joseph Berger, Santo F. Camilleri, Sanford M. Dornbusch, and Morris Zelditch, Jr., for their comments and suggestions.

The author's special gratitude is due Mrs. Joan Blackett, Mrs. Harriett Ferziger, and Miss Camille Wrathall, who typed the difficult manuscript.

Finally, and most important of all, the author wishes to express his deepest appreciation to Dr. Elizabeth G. Cohen for her many roles — colleague, assistant, and devoted wife.

Stanford, California
January, 1962

Bernard P. Cohen

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CONFLICT AND CONFORMITY:

A Probability Model and Its Application

Chapter 1

MATHEMATICAL MODELS AND THE STUDY OF CONFORMITY

1.1 Introduction

This investigation concerns conformity behavior as it occurs in highly abstract, highly controlled situations. We want to understand and describe the core of the conformity phenomenon before it is overlaid with the complexity of other variables operating in less controlled settings. We believe these experimental situations, particularly Asch's experiments (Asch, 1951, 1952, 1956), capture the core of the phenomenon, and represent an appropriate starting point for constructing an explanatory theory. We believe that close examination of the behavior which occurs in the Asch setting, as well as careful scrutiny of the properties of the setting itself, will contribute to the development of such a theory.

Systematic investigation of any phenomenon requires adequate conceptual tools, in this case the development of a formal, mathematical model to describe behavior in this experimental setting. The major part of this investigation has been devoted to the construction and testing of a probability model for conformity. Once we are satisfied that our model provides an adequate description of the phenomenon, the model itself becomes a vehicle for the analysis of other similar experiments. This monograph then is a description of the development, testing, and application of a probability model that we term the "conflict model."

Before introducing the conflict model, however, we must dispose of several questions relating to the development of the model. Some of these are quite general and concern the research strategy involved in model building; others are specific to the choice of this experiment as the basis for formalization. The next section, 1.2, will examine briefly the role that mathematical models can play in social science research. Section 1.3 will provide the first look at the experimental situation, indicating some of the questions it raises and pointing out some of the factors that make it strategic for the development of a mathematical model. A summary of this chapter and the plan for the remainder of the monograph will appear in Section 1.4.

1.2 Models as a Tool for Social Science

(1) Why should a social scientist look to a mathematical model for aid in advancing his knowledge of a substantive problem? (2) What is a mathematical model, and what results can it yield? (3) What have some of the current social science models accomplished? Some consideration of these issues is necessary background for the evaluation of the model to be presented in this report.

Let us turn to the first question.

One of the problems confronting much of social science and particularly sociology is the gap that separates theory and observation. On the one hand, the abstract and necessarily general character of theory often discourages the empirically oriented sociologist from designing his research to test a body of principles. On the other hand, the concrete and specific nature of a particular empirical study frequently prevents the theorist from seeing the study as an instance of the propositions he advances. The gap, with all of its implications for the failure to cumulate knowledge, will remain as long as there are no serious efforts to formulate unambiguous "chains of reasoning" that link general principles to observational statements. While we social scientists have devoted a great deal of attention to the problem of obtaining interobserver agreement on the observations we take, we have not shown similar concern with obtaining such agreement on the meaning of the assumptions we make or on the consequences that follow from these assumptions. Thus, when a theoretical principle is advanced, there are no restraints on the operations which may be used to "test" that principle; and when a particular empirical finding is reported, there are no directions to indicate how that finding may be generalized. The empiricist who sneers at a theory, "It isn't testable!" and the theorist who jeers at an experiment, "It isn't generalizable!," miss the central issues. These issues involve restricting, by explicit argument, the observations comprising the subject matter of the theory and tying, again by explicit argument, results obtained from any set of these observations to the abstract principles of the theory. Until such links are established, the social sciences will consist largely of a body of untested principles and a catalog of ungeneralized observations.

One function of a mathematical model is to present an explicit chain of reasoning from assumptions to observations. The conflict model developed here formalizes some general assumptions about conformity, and, as a formal description, it explicates the chain of reasoning from these assumptions to the observations collected in experiments.

Formal models would perhaps be more congenial to social scientists if they could be stated solely in verbal terms. But to expect these chains of reasoning to be developed in purely verbal terms is too sanguine a hope in view of the complexity of ideas with which we deal and in view of the difficulties with language itself. To be sure, the assumptions of the conflict model will be stated verbally. But any attempt to draw out the implications of these assumptions is altogether too cumbersome. For one reason, as only the most cursory familiarity with sociological discussions of conformity quickly demonstrates, social science terms carry connotations beyond those intended in our assumptions, and these surplus meanings would continually lead us astray in our effort to derive implications. Hence, it is necessary to translate our assumptions into a contentless symbolic language that possesses rules for logical manipulation, manipulate these symbols to arrive at conclusions, and then retranslate these symbolic conclusions into the language of our observations. It is important to stress the fact that the symbolic language is a basic tool which adds nothing to the content of the model. If translating our notions into symbols added content to these ideas, it would defeat the purpose of the translation.

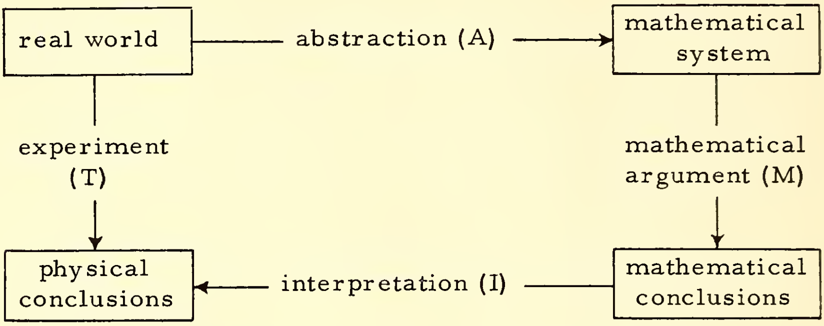
Symbolic languages can take various forms. Physical replicas, computer programs, and mathematical systems are among the most common means of symbolic representation. In the present model for conformity we employ concepts and results from a part of mathematics, probability theory.

The earlier discussion of motives for model building in the social sciences briefly alluded to several important features of a mathematical model. Now we shall focus directly on these features. Earlier we asked, "What is a mathematical model and what results can it yield?" Phrasing these questions simply can mislead us into believing that there are simple answers. Such is not the case; there are many unresolved issues concerning the nature and function of mathematical models, most of which are beyond the scope of the present discussion. It is possible, however, to outline several important and agreed-upon properties of these models.

A mathematical model is the application of a mathematical system to an empirical phenomenon. Coombs, Raiffa, and Thrall (1954, p. 21) discuss the mathematical system and the process by which it is related to the empirical world:

> A mathematical system consists of a set of assertions from which consequences are derived by mathematical (logical) argument. The assertions are referred to as axioms or postulates of a mathematical system. They always contain one or more primitive terms which have no meaning in the mathematical system. . . .

Our view of the role that mathematical models play in a science is illustrated in (the following diagram):



> With some segment of the real world as his starting point, the scientist, by means of a process we shall call abstraction (A), maps his object system into one of the mathematical systems. . . . By mathematical argument (M), certain mathematical conclusions are arrived at as the necessary (logical) consequences of the postulates of the system. The mathematical conclusions are then converted into physical conclusions by a process we shall call interpretation (I).

In their diagram, these authors point to the comparison of the mathematical conclusions with physical conclusions arrived at by experimentation; this comparison serves to test the goodness of fit of the model. The particular importance of the processes of abstraction and interpretation, (A) and (I) in the previous diagram, should be noted. These processes relate the mathematical system to the empirical phenomenon through a series of identifications and coordinating definitions. In the present investigation, we attempt to capture the essential ingredients of the conflict process, which leads to conformity or deviance, in the mathematical system we choose; then we derive conclusions about abstract variables, which are then related to the experimental measurements we actually make.

Bush (1953, p. 2) suggests six possible results that a mathematical model may yield:

1. [A model] provides a shorthand for communication.
2. It may organize assumptions and uncover hidden ones.
3. It may suggest new experiments or new theoretical ideas.
4. It may display the complexities of a problem.
5. It may predict the results of experiments.
6. It may serve to relate different problems.

While most of these are self-explanatory, they do require some comment. With the model and a small number of numerical quantities estimated from the data, that is, parameters, it may be possible to describe a large number of experimental results. In the present study, the model and the parameters could describe time changes in conformity behavior, the amount of conformity at any point in time, total performance, that is, total number of conforming responses, of the subjects, and other statistics drawn from the data. This fact also illustrates an essential difference between constructing a model and "fitting a curve" to a set of data. In fitting a curve, only a single assumption is involved, namely, that a particular function (exponential, logarithmic, etc.) describes a set of data. Even if this assumption is adequate, that is, if one can find a function to fit the data, nothing is known about the other properties of the data. In this research, for example, it is quite possible to find a best-fitting function to describe the changes in the proportion of subjects who conform, but such a function would not predict other results such as the frequency distribution of conforming responses for the whole experimental situation.

Bush (1953) feels that if a model accomplishes any of the six results he lists, it is worth considering. Most of the models that have appeared in the social science literature fulfill this weak criterion. We can illustrate some of these accomplishments by examining several social science models.

Simon (1952) and Simon and Guetzkow (1955) have attempted to formalize current theories of small group behavior. The first report presents a model for Homans' conceptualization of the interrelations of sentiment, interaction and activity (Homans, 1950), whereas the later work deals with Festinger's theory of informal social communication (Festinger, 1950). Both of these models were intended to clarify theory rather than to describe existing data. They succeed in making explicit several assumptions that were implicit in the verbal statements of the original authors.

In the present author's opinion, there are two basic difficulties in Simon's approach.[1] The first involves the use of "classical" mathematics, which, in its assumptions of continuous variables, requires more sophistication in measurement than is currently present in the social sciences. The second difficulty concerns the relations of the abstract system to the empirical world. The elements of the abstract system are meaningful terms such as "friendliness," "interaction," etc., and Simon does not further identify these elements with well-defined sociological concepts. In failing to do so, he misses the opportunity to clarify some of the notions he has taken directly from Homans. It is doubtful that his model would apply to all of the current operations defining these everyday terms.

Other authors avoid the difficulties present in Simon's work by applying other branches of mathematics not involving continuous variables and by identifying the elements of the abstract system with observables rather than conceptual entities. Thus Anderson (1954) presents a probability model for analyzing time changes in attitudes in which he identifies the states of a Markov chain[2] with response classes on an attitude questionnaire. As Anderson points out, it is desirable to construct models based on well-developed theory; in the absence of such theory, however, it is safer to coordinate the model to observables rather than to concepts for which there are conflicting operational definitions.

It should be noted that Simon and Anderson are dealing with somewhat different problems. One wishes to explicate existing theory, and the other wishes to describe a set of data in an area in which theory is relatively scarce. Because we desire to describe experimental data, the present approach, although it contains substantive elements, will be more similar to that of Anderson than it is to Simon.

We should mention a few examples of probability models for social science problems to illustrate some of the fruitful outcomes of this approach.

Mathematical learning theory is the area of most widespread interest in formal models.[3] Bush and Mosteller (1955) have developed a model for learning which has been applied to data more extensively than any other model. Although the model does not incorporate any specific learning theory, it describes a wide range of phenomena that are of interest to learning theorists. The model is quite successful in describing acquistion in both human and animal subjects, and various types of learning such as rote learning, imitation, and avoidance learning have been analyzed with the model. In addition, the model has led to numerous experiments, including an effort to apply a special case of the general model to group behavior (Hays and Bush, 1954).

Another major line of development in learning stems from the work of Estes (e.g., Estes, 1950). The Estes-Burke (1955) model formalizes a "stimulus sampling theory" and yields specific numerical predictions for quantities usually examined in learning experiments. Suppes and his associates have expanded on this type of model and applied their results to group learning (Atkinson and Suppes, 1958) and to a situation similar in some respects to the Asch conformity experiments (Suppes and Krasne, 1959).[4]

Anderson's paper (1954), which we have already mentioned, represents an effort to construct a baseline model for attitude change in order to discover systematic deviations from this baseline. Thus, for example, he assumes that an individual can be in one of two states; that the state on "trial" n + 1 depends only on the state on "trial" n (which defines a Markov process); and that the probabilities of movement between states are constant over all "trials." When he considers Republicans and Democrats interviewed at several points before the 1948 election, he finds that more switching between parties occurs at the time of the party conventions. Finding such a deviation from his model suggests classes of events for special study as well as further assumptions necessary to improve the fit of the model to the data.

Another interesting application of Markov processes is the study of Leeman (1952), who develops a model for sociometric choice. The important feature of this investigation is the highly abstract experiment he designs to test this model. His work indicates the possibility of new and relatively simple experiments arising from attempts to test a model.

A quite different branch of mathematics, graph theory, is the basic tool in Cartwright and Harary (1956). These authors present a formal definition of structural balance and examine the consequences of their definition. The importance of this model lies in the fact that structural balance is the key concept in Heider's theory of cognitive organization (Heider, 1946). Cartwright and Harary are able to clarify and extend Heider's notion through the introduction of graph theory.

Other areas in which models have been developed include such diverse fields as psychoanalytic displacement (Bush and Whiting, 1953); labor mobility (Blumen, Kogan, and McCarthy, 1955); language and information (Miller, 1952; Miller and Frick, 1949; Newman, 1951); and pecking order (Rapaport, 1949a, 1949b, 1950). The work of Blumen, Kogan, and McCarthy, although it does not represent a model that adequately describes the data of the study, presents an excellent example of how a research team develops and applies a sequence of models to an important social science problem, that of the flow of workers through the industrial structure of the United States.

We have presented these examples to illustrate some of the areas of application of mathematics and also some of the possible benefits of such application. In addition, the studies considered suggest something about the strategy of model building, namely, that efforts to develop a model are most appropriate when there is sufficient theory to formalize or a large body of reliable data to describe. Studies such as Anderson and Blumen and associates further indicate that the process of developing a model involved examining a sequence of models, beginning with simple, baseline models and proceeding from these as more is learned about the data. As Chapter 3 will show, this sequential procedure was followed in the present study.

1.3 The Asch Situation: A Strategic Area for Model Building

The essential features of the Asch situation are well known because its simplicity has led to extensive use both in research and as a classroom demonstration. The procedure requires a naive subject to judge the equality of line segments in the face of the clearly incorrect responses of an instructed group of peers. Typically, a subject is asked to make eighteen such judgments of a variety of stimulus lines. The correct response for each such judgment is clearly discriminable, but on most of the trials the subject is confronted with a response which diverges from his sense perception and which is unanimous. The subject then must choose between reporting the correct line or yielding to the pressure of his peers.

The data are also extremely simple, consisting of a sequence of correct and yielding responses for each subject. These sequences are usually summarized for a sample of subjects by giving each subject a "conformity score" based on the number of times he has yielded to group pressure and presenting a frequency distribution of these scores. The resulting distributions show remarkable similarity from study to study even though the types of subjects used vary markedly. In addition to replications of Asch's original study, numerous investigators have related these conformity scores to other variables such as personality tests, modalities of judgment, and characteristics of the confederates.

Here then is a situation which because of its simplicity is readily repeatable and which has generated widespread interest among students of conformity. Furthermore, there is general agreement that the behavior elicited is not flippant but a significant manifestation of a conformity phenomenon. These properties of the Asch situation fulfill what we regard as prerequisites for the development of fruitful models: (1) Numerous replications indicate that the phenomenon is not transitory. (2) There is considerable appreciation of the importance of the phenomenon as well as some understanding of its nature. (3) There is a desire to make further use of the experimental setting as a standardized test situation in which to study the effects of related variables. Finally, and perhaps most important, a number of questions about the situation and the behavior it elicits remain unanswered.

While the experimental situation meets prerequisites for fruitful model building, this is not sufficient to motivate a substantive worker to construct a model. What of our understanding of the phenomenon itself? A model must add to this understanding. If the model formalized a theory and the symbolic language were used to derive implications of this theory, then its potential substantive contribution would be clear. The present model, however, does not formalize a theory; its purpose is descriptive.[5] But a successful descriptive model can be an important antecedent to the development of theory. This can best be illustrated by considering some of the issues that arise in connection with this experimental situation and model building.

It is unfortunate, in view of the interest in Asch's work, that there have not been any attempts to explain in other than a correlational sense the behavior occurring in the Asch experiment. Neither have there been many attempts to analyze properties of the situation nor to formulate the abstract concepts that could be used to generalize the results of Asch-type situations to other conformity experiments. While there has been a cumulative series of studies, primarily these studies are related only in the sense that they consider the same dependent variables; they are not tied together in a common effort to develop and extend a theory.

One feature of this conformity research which hinders the development of theory is the concentration on "frequency of yielding" as a crucial variable. Even a simple laboratory experiment has a number of component variables. Instead of devoting full attention to a "total performance" measure such a frequency of yielding, one could ask questions such as: What happens to the subjects' responses through time? Do subjects become consistent in their responses? If subjects tend to stabilize, does this equilibrium point occur after a few judgments, or is group pressure effective even after a large number of trials? While it is true that Asch considered these questions, subsequent research has by and large ignored them. Furthermore, Asch treated the indices he computed as if they were independent and unrelated analyses. In the absence of theory, his best assumption probably was that the variables implied by the preceding questions could be treated independently. If a descriptive model could indicate the interrelations among these variables, it might clarify what aspects of the phenomenon should be taken as elements of a theory.

Another factor related to the preoccupation with conformity scores is the fact that the distribution of these scores shows large individual differences in frequency of yielding. On seeing such a distribution, many investigators assume that it is related to enduring individual differences in such areas as personality traits, intelligence, and the like. Hence, the literature is filled with attempts to relate behavior in this type of experiment to measures of personality. But is this assumption necessary? There are many other variables whose operation could give rise to the type of response distributions observed. One aim of developing a model is to set down explicitly the assumptions underlying the experimental situation in order to determine those which are necessary and those which are excess baggage.

Any experiment has some elements which are essential to the production of the phenomenon and some elements which can easily be modified or eliminated without altering the main experimental effect. This situation is no exception, but the basis for distinguishing between peripheral and central features of the situation depends on the purpose of the research. For example, if one is interested in demonstrating that the "conformity effect" is independent of the stimuli used, then a variety of stimuli is a central feature of the design. The conflict model, however, is concerned with describing the core of the conformity phenomenon, not its variability; hence any variations that can be removed without removing the "conformity effect" become peripheral features of the procedure. To build a model, it is necessary to abstract, because the model is an idealization. The process of abstraction forces a decision regarding what are central and what are peripheral elements of the experiment. In this research, we were forced to consider what variables could be removed without noticeably reducing the amount of conformity observed. We found that we did not need to vary the stimuli presented and, in retrospect, saw that other Asch-type research could have omitted this variable and achieved more stable relationships. Model building, then, can also contribute to a clarification of the experimental procedure.

These examples illustrate the model's utility for clarifying and ordering a phenomenon. In addition, the model provides a basic tool for another aspect of the problem — the study of a social process. An important objective of the model is to describe the changes in conformity behavior which take place through time. Since this concern with process represents a different orientation from most previous conformity research, it is worth while to examine the problem in some detail.

Coleman (1960) has suggested that small group research generally has concerned itself with the demonstration of single relationships rather than with the detailed examination of processes. His statement is especially applicable to studies of conformity. We may call those studies focusing on single relationships "structural" studies, since they deal with structural conditions (such as characteristics of a group, of a person, of a social climate, etc.) that lead, in the case of studies of conformity, to more or less norm-oriented behavior. The typical design of this type of research consists of two experimental conditions established by some classification of subjects or groups; each of the experimental conditions is subjected to the same social process, and then summary measures of behavior are compared for the two groups. The important contribution of structural studies is the demonstration of the conditions under which we observe a particular phenomenon.[6] Although there have been significant results in this area, there is a difficulty present in studying conformity solely in structural terms.

The effects of a particular structural variable are usually examined at a point in time, and it is usually assumed that the state in which the subject is found at that point in time is a terminal state. For example, a thirty-minute discussion is followed by an opinion questionnaire to measure how much the subject changed his opinion as a result of group pressure; the answers that the subject gives are assumed to represent the answers he would give at other points in time. Although most investigators are aware that this is a tenuous assumption and that the terminal states observed might differ if measurements were taken at other stages of the process, it does seem that the problem deserves additional consideration. The question may be phrased as follows: When are the observed states temporary and when are they "absorbing"? An illustration might serve for the moment to define temporary and absorbing. An individual is in a group for thirty minutes, the last twenty of which he has conformed to the majority opinion. Looked at after thirty minutes, conformity is a terminal state for that individual. Suppose, however, that the group continued for thirty minutes longer, and during that time the same individual rejected the majority opinion for the whole thirty minutes. Then conformity was only a temporary state for that subject. If the subject began to conform and continued to conform no matter how long the group continued, conformity would be an absorbing state for that individual.

In addition to measures of terminal behavior, for example, opinion at the end of a discussion, some investigators, as we have noted, have compared total performance measures of groups that differ on some characteristic. Such a measure as the number of communications in a discussion period is an example of the latter approach. But the variation in time of some of these measures is not often considered. This variation in time could be as significant as total performance measures. Studying time changes in conformity may lead to increased understanding of the factors producing such changes; hence such investigation could expand our knowledge of the conditions under which conformity behavior occurs. For example, a person who vacillates between conformity and nonconformity may differ from a person who is a consistent conformer or nonconformer, with respect to motivation, attitude toward the group, personal security, etc.

Several investigators have studied social processes,[7] but research on time changes in conformity has met two main obstacles. First, it is difficult to construct appropriate experiments because taking repeated measurements often introduces spurious factors. Second, once these measurements have been taken through time, there are relatively few means of summarizing them which (1) preserve the time-series character of these observations and (2) are comparable from study to study.[8] The present study, however, indicates that it is possible to deal effectively with these problems. We obtain repeated measurements in the experiment without the subjects being acutely aware of this repetition.[9] And we can summarize these measurements by constructing a process model and treating other aspects of the phenomenon as consequences. In this study, we postulate an underlying process that generates the observed process as well as total performance scores and other properties of the data. The postulated process represented by the conflict model is much simpler to describe and summarize than the observed process. Since the model fits the observations reasonably well, it provides the means for describing the observed process.

The major reason, then, for our belief that this experimental situation is strategic for model building is that model building is strategic for our understanding of the process which the experiment involves. The model enables us to focus on what we regard as the crucial aspect of the experiment, an aspect that has not been adequately treated, namely, the time changes in conformity behavior.

1.4 Summary

This chapter has presented the major considerations behind the decision to develop a mathematical model for situations similar to the Asch conformity experiments. We have not attempted to develop a systematic analysis of the properties and functions of mathematical models, or even a systematic analysis of the theoretical possibilities inherent in the Asch situation. Rather we have posed a general problem confronting the social sciences — the discrepancy between abstract principles and specific observations — and have described one means of coping with that problem, that is, the development of mathematical models.

While the discussion of the properties of a model and the consideration of current social science models clearly indicate that models can and do serve a variety of purposes, these purposes all represent some form of codification. The codification may deal with a theory, or with the elements of a phenomenon; it may take place at different levels of abstraction; it may be more or less fruitful. But a model is an effort to organize ideas and explicate the relations among these ideas. As an organizing tool, a model serves as a baseline from which to proceed to cumulate knowledge.

The second issue considered was the strategic position of the Asch situation. This dramatically simple experiment has received widespread attention. In fact, it is one of the few social experiments for which there are numerous replications. Despite the remarkable consistency of experimental findings, there have been no efforts to organize and systematize what is going on in the Asch situation. At present, we are not ready to formalize a theory about this phenomenon, but some codification of the situation can contribute to the development of theory by describing the interrelations among the variables present and by generally clarifying the experimental procedure.

The last issue discussed — the problem of studying social processes — has broad implications. Many of our notions are process notions. For example, "If the frequency of interaction between two or more persons increases, the degree of their liking for one another will increase. . . ." (Homans, 1950) Investigation of such propositions has been handicapped by the difficulty of constructing appropriate experiments and the lack of conceptual tools for handling process variables. We believe that (1) experience with the Asch situation indicates that it is a fruitful prototype for process experiments, and (2) the present study points to mathematical systems which provide adequate descriptive tools for dealing with process variables.

We have indicated the motivation for this research. The task now is to describe the steps by which models are constructed, the conflict model itself, the fit of the conflict model to experimental data, and the ways in which the model may be extended.

NOTES

1. See Coleman (1960) for a detailed analysis of Simon's model.

2. A finite Markov chain is a special type of probability process that involves a number of "states" and movement through these states probabilistically in a sequence of steps. If at any point in time the process is in one state, call it a_i, it moves on the next step to another state a_j, with probability p_{ij}. These probabilities p_{ij} are called transition probabilities. The process is completely determined by specifying these transition probabilities and the state in which the process begins. For an introduction to Markov chain theory, see Kemeny, Snell, and Thompson (1956), Chapters 4 and 5.

3. For an excellent review of the background of mathematical learning theory written for those not familiar with learning theory, see Bush (1960).

4. The Suppes-Krasne experiment was designed primarily as a learning situation and was analyzed from that point of view. Since reinforcement was built into the experimental procedure, their study is not directly comparable with the present work.

5. Although the conflict model is based on substantive notions, these notions are extrinsic; that is, they are guides in the development, but they do not imply any particular model as a consequence.

6. Thus Sherif (1936) showed that when the stimulus field is ambiguous, subjects may be influenced to perceive it in terms of an experimentally induced norm; Asch (1951) demonstrated that yielding to group forces was a function of (1) the character of the stimulus situation, (2) the character of group forces, particularly unanimity, and (3) the character of the individual. Festinger, Schachter, and Back (1950) and Festinger and Thibaut (1950) found that uniformity in a group increases as the cohesiveness of the group increases. Although this last generalization is phrased in process terms, it was studied with a structural design. That is, three conditions differing in cohesiveness were experimentally created and then compared on the amount of opinion change.

7. The efforts that have been made to study social process should not be overlooked. Bales's Interaction Process Analysis (1950) provides a tool for a wide variety of content areas; his consideration of phase movement (Bales and Strodtbeck, 1951, and Bales and Slater, 1955) represents an attempt to uncover the time changes in group discussions. Schachter (1950) has examined time changes in communication to the deviant, and Asch has examined his data (1956) to see if there were systematic differences between halves of the experiment.

8. It is possible, of course, to compare two time series graphically, but such analysis is often unsatisfactory, and the analyst is led to fitting curves to the two sets of observations in order to simplify his comparison. But then he has taken a step toward developing a model.

9. We believe that many of the spurious factors arising in situations of repeated measurement result from the subject's awareness of what is taking place. A particular concern is with the subject's loss of motivation when he realizes that he is doing the same thing over and over again. Although some subjects are aware of the repetition in the present version of the Asch experiment, loss of motivation does not appear to be a problem.

Chapter 2

DESCRIPTION OF THE EXPERIMENTS

2.1 Introduction

One of the strategic considerations discussed in the previous chapter was the presence of a sufficiently large body of data. The regularity of the phenomenon one wishes to describe is a major reason for this concern. In addition, some understanding of the conditions under which the phenomenon occurs and of the factors that affect it is a prerequisite to any formalization. It is unlikely that a mathematical model will have much predictive or much heurisitic value if the phenomenon is either transitory or little understood. Fortunately, a large number of investigators have used the procedures that Asch originated to study conformity behavior; as a result of this previous research, we can have confidence in the reliability of the conformity resulting from these procedures. Furthermore, many of the insights of these investigators, particularly Asch, can aid in the present attempt to develop a formal description of the process involved in these experiments.

Since Asch's research is central to the present study, the next section will discuss some of Asch's experiments on conformity and independence. It will describe his procedures and present some of his results. Section 2.3 will indicate the experimental procedure used in the present investigation and also point out where this procedure departs from Asch. Finally, Section 2.4 will examine some of the results of the present author's experiments and compare these results, where possible, with those Asch obtained.

2.2 Asch's Experiments on Conformity

The object of Asch's research was to study some of the conditions "that induce individuals to remain independent or to yield to group pressures when these are contrary to fact" (Asch, 1952, p. 451). He has succeeded in demonstrating some of the factors producing conformity as well as those aiding an individual to remain independent[1] of group pressures. For the present work, both the procedures Asch developed and some of his findings are of interest.

Central to nearly all of Asch's experiments (1951, 1952, 1956) is the presence of a minority of one in the midst of a unanimous and incorrect majority. A naive subject enters into what is ostensibly a group perception experiment, but the other members of the group are role players whose behavior has been prearranged. The experimental task consists of a sequence of judgments which must be made orally, and for which there is a correct response. The role players give their judgments orally; on some judgments or trials, these confederates are correct, and on others they are incorrect. On all trials, the confederates are unanimous. Usually, six of the seven[2] role players precede the naive subject, so that he is aware of their responses before he makes his own judgment. On those trials when the confederates are incorrect, there is a conflict in the subject between his tendency to respond correctly and his tendency to yield to social pressure.

Typically, Asch's experimental session consists of presenting a subject with a sequence of 18 judgments. At each trial, the experimenter shows two cards, on one of which appears a single line, the "standard," and on the other of which are three "comparison" lines. The task is to match the standard with the comparison line to which it is exactly equal in length. The comparison card always contains an equal comparison line, and the correct match is always clearly discriminable. The two incorrect comparison lines are of different sizes, varying from $\frac{1}{4}$ to $\frac{3}{4}$ of an inch longer or shorter than the correct line. Hence, one incorrect response represents a more extreme error than the other. The confederates and the naive subject have three responses available to them: correct, a "moderate" error, and an "extreme" error; trials are classified according to the type of response the confederates have been instructed to make. Those trials on which the majority responds correctly are termed "neutral," whereas the trials on which the confederates are incorrect are called "critical" trials. The critical trials are further subdivided into "moderate" and "extreme," depending on whether the prearranged response is a moderate or an extreme error. It is the critical trials that are of primary interest; the neutral trials are included to allay the subject's suspicion and to indicate that the majority is capable of giving a correct response. The subject's behavior on the critical trials, whether he conforms to the majority or remains independent, constitutes the major data of the experiment.

Moderate, extreme, and neutral trials are interspersed in the same experimental sequence. In addition, the trials differ with respect to the stimuli judged, which vary as to size of line to be matched and as to the discrepancy between the correct match and the incorrect lines. Variation of the stimuli and the group response are incidental to the main purpose of the experiments and are included to prevent monotony and the suspicion that might occur if the trials were too highly repetitive. Furthermore, these variations provide a broader sample of the conditions under which social pressure could induce a subject to conform to the majority. While Asch did examine some of the systematic effects of varying the stimulus, these effects were of less interest than the fact that some conformity occurred with each stimulus. Although this factor is not a serious problem in relation to Asch's findings, anyone interested in using his procedures for other purposes, such as studying time trends, should consider the issue seriously. For example, variability in the subject's behavior because of changes in the majority's response might far overshadow any time trend in the subject's judgments. We shall return to this problem in the next section.

Asch has reported the results of several experiments. His latest monograph (Asch, 1956) summarizes a large portion of his work on conformity and represents the most recent and most complete statement of his views of the phenomenon. Therefore, this discussion will follow his monograph.

The monograph devotes considerable attention to Experiment 1, the experiment which has the largest number of subjects (123) and for which the most intensive analyses have been made. This experiment is actually a replication of the same procedure on samples of students drawn from several colleges. Since these samples do not differ significantly from one another, Asch combines data for all of them in his report.

Experiment 1 consisted of 18 trials; on each trial the subject had to judge which of three lines equaled a given line. The response of six of the seven role players, however, preceded the subject's, and this response was correct on six trials, a moderate error on six and an extreme error on six trials. The order of the trials was such that no more than two trials on which the confederates made the same type of response were adjacent. Six different sets of lines were used, but for the critical trials there were only three sets of stimuli. Of these three sets, each was used on four trials, twice when the majority made a moderate error and twice when they made an extreme error. Furthermore, the halves of the experiment were symmetrical; that is, the same stimuli occurred in the same order with the same group response in each half of the experiment. Following the experiment, Asch interviewed his subjects intensively. Asch reports what he calls "quantitative" and "qualitative" findings, the first based on the experimental performance and the second on the interview results. His quantitative findings include (Asch, 1956):

1. Whereas individuals [in a control group where there were no instructed role players] judged the relations in question with almost complete accuracy, "the majority succeeded in deflecting one-third of the minority estimates in its direction." (P. 24.)
2. Three-quarters of the subjects made at least one error in the presence of the majority. (Table 3, p. 10.)
3. Individual subjects showed a high degree of consistency; those who yielded at the beginning of the experiment tend to conform on most trials, while those who were independent early in the sequence were likely to remain independent. (P. 24.)
4. There were great individual differences, subjects ranging from complete independence of the majority to complete submission to it. (P. 24.)
5. There was no evidence of temporal trend in the tendency to conform. (P. 15.)

The qualitative material supports the view that the situation is one of considerable conflict. Subjects were seriously concerned over disagreements, and this concern bore no relation to performance (Asch, 1956, p. 30). In addition, most subjects reported that they knew the correct response despite the fact that they gave the majority response (Asch, 1956, p. 32), supporting the view that this phenomenon is a distortion of response rather than a distortion of perception. To put this another way, it is a case of overt compliance.[3] Finally, most subjects who yielded to the group gave inaccurate estimates of their performance, underestimating the number of times they went with the majority (Asch, 1956, p. 34).

Several of Asch's points are particularly relevant to the present research. First, as Asch notes, the strongest force acting on the individual is one toward independence; most of the responses were correct. However, the fact that most subjects make at least one error raises the question: "If a subject makes few errors, when are these most likely to occur?" The model to be considered here deals with this problem. The model also incorporates the third finding just listed; that is, the model is, to a large extent, based on the fact that Asch's subjects were highly consistent in their responses. Because this consistency is crucial to the model, we shall again consider it after we have examined the model in detail.

Asch's findings with regard to individual differences and temporal trends deserve special attention. We shall first consider temporal trends. Asch investigated temporal trends by comparing halves of the experiment. Since these were symmetrical, the procedure is reasonable; it is, however, a relatively crude form of analysis which would uncover only extremely pronounced trends. Furthermore, as Asch recognizes, and as Chapter 3 of this work shows in detail, the lack of a group trend could be owing to compensating individual trends. Another problem concerns the length of the experiment; it could be that too few trials were included to allow a consistent trend to appear. To meet this problem, Asch conducted an additional experiment (Experiment 5), which contained 36 critical trials. In this analysis, he compared thirds of the experiment. The number of subjects in the experiment, however, was too small to draw reliable conclusions. Finally, some of the extrinsic variables, for example, variations of stimuli, may have prevented the appearance of clear-cut trends. The experimental design of the present research represents an effort to overcome some of these difficulties. In addition, the model will offer analytic possibilities that are more efficient than examining blocks of trials.

In discussing individual differences, Asch mentions two possible interpretations (Asch, 1956, p. 51):

> [Individual differences] may be the product of momentary, episodic circumstances; if so, they are of minor import. Or they may be a function of consistent personal qualities which individuals bring into the experimental situation and which have as decisive a bearing on their actions as any of the other conditions we have studied.

In this context, Asch is attempting to account for individual differences in the frequency of yielding to the majority. To Asch, the importance of individual differences lies in enduring personality differences. But it may not be necessary to assume individual differences prior to the outset of the experiment in order to explain the observed differences in the number of errors. If such is the case, it does not mean that individual differences are unimportant, but rather that one must look at other aspects of the data for manifestations of different personal characteristics. For example, two persons with differing attributes may arrive at the same number of yielding responses by a similar reaction to that aspect of the process. However, their personality differences might be manifested in the way in which they arrive at a total behavior score; in an extreme case, one subject might make all his errors at the beginning of the experiment, while the other made his at the end. The question we raise, then, concerns those aspects of the data which can be explained by a model that does not assume individual differences. Chapter 4 will return to this question.

Asch replicated the major findings of Experiment 1 in several other experiments. In addition, he found that the effect of the majority remained large when the type of sensory judgment was changed, that this effect decreased when the subject "was relieved of the necessity of announcing his dissent openly" (Asch, 1956, p. 69), and that giving the subject a partner who was instructed to respond correctly dissipated the majority effect. Furthermore, he showed a relationship between the number of errors and both the size of the lines to be judged and the descrepancy between the correct and incorrect alternatives.

The essential experimental conditions, according to Asch, are the introduction of an unambiguous stimulus, the presence of a unanimous majority, and the necessity to respond publicly. Other investigators have designed similar experiments which do not make use of role players and, for that reason, are more economical. For example, they use pencil and paper techniques, tape recordings, or other devices to simulate the behavior of the others in the group. (For example, Crutchfield, 1955.) These procedures, however, do not fulfill what Asch regards as the essential conditions and show somewhat weaker effects. In the present research design, we have modified some elements of Asch's procedures but have attempted to keep the central features. The next section will describe these modifications and will also consider how successful we have been in producing an effect similar in magnitude to that which Asch obtained.

2.3 The Present Experiment

This experiment consisted of two conditions, each involving a naive subject confronted by a unanimous and incorrect majority and differing only in the response of this majority. In basic structure, each condition of this experiment is similar to Asch's work. This investigator's procedures, however, differ in several important details from those of Asch.[4] This section will describe these differences and discuss their implications.

Since one concern of this study is to describe time trends, this author increased the number of trials to allow the appearance of any time trends that might exist. Where Asch primarily used 12 critical trials, each condition of the present experiment had 36 critical trials. The other distinctions can be discussed under two headings: properties of the sequence of stimuli and properties of the sequence of majority responses.

Asch systematically varied the properties of his stimuli from trial to trial. Typically, he varied the size of the standard, the positions of the comparison lines with respect to one another, and the relation of the size of the correct line to the size of the comparison lines. For example, Experiment 1, already described, using six different standards, had the correct comparison line in each of the three possible positions on its card, and varied the size of the discrepancy between correct and incorrect lines. Even in the absence of role players, the subject for Experiment 1 had to make six separate judgments of varying difficulty, although all above threshhold. The present investigator controlled the properties of the stimulus sequence. In both conditions, the same standard and the same comparison lines were presented on every trial.

The standard was always 8 inches long; the comparison lines were $6\frac{1}{4}$, 8, and $6\frac{3}{4}$ inches long. The comparison lines were arranged in that order, from left to right. The size of the stimulus lines, their relations, and their physical positions on the card were constant from trial to trial. Thus the subject had to make exactly the same judgment on every trial.

As in Asch's studies, the subject could make one of three possible responses in this situation: He could choose the 8-inch comparison line, making a correct response; he could choose the $6\frac{3}{4}$-inch line, making a moderate error; or he could choose the shortest line ($6\frac{1}{4}$ inches long) and thus make an extreme error. Although these are three mutually exclusive response classes, most of the analysis of the subject's behavior will be in terms of just two classes, "correct" and "incorrect," the latter including both moderate and extreme errors.[5]

The only trial-to-trial variations in the stimuli were designed to prevent the subject from realizing the deceptive nature of the situation. These devices were alternating the side on which the card containing the standard line was presented and changing the labels of the comparison lines. On half the trials the standard was to the right of the comparison lines, and on half it was to the left. The position of the standard on a given trial was assigned using a random number table. The three comparison lines bore the identification A, B, and C on the first trial, D, E, and F on the second trial, etc., and S, T, and U on the seventh trial. The sequence of labeling was periodic with a seven-trial period.[6] Varying the stimuli attempted to eliminate auditory reinforcement to the visually perceived sameness of the stimuli. It was felt that if the subject were aware of the sameness of the stimuli, he would doubt that the purpose of the experiment was to study visual perception and hence would be likely to realize its true purpose.

Building these two variables into the stimulus sequence could ntroduce systematic changes in the subject's behavior. It was hoped, however, that such effects would not occur, since the properties of the stimuli that varied were irrelevant to the discrimination task.

In Asch's research, the sequence of majority responses included moderate, extreme, and neutral trials. Experiment 1, for example, had the following sequence:

N, N, M, M, N, E, M, E, E, N, N, M, M, N, E, M, E, E

where N indicates a neutral trial, and M and E signify moderate and extreme trials, respectively.[7] In the current investigation, the author decided to hold the majority response constant once the critical trials began. He felt that the confederates exert a differing amount of social pressure on moderate trials from that exerted on extreme trials. Asch's data tend to support this view, at least in the effects of moderate and extreme trials; more subjects yielded to the majority on moderate than on extreme trials.

The distinction between moderate and extreme trials is one of important conceptual significance, particularly if the interpretation is correct that these types of trials differ in the amount of social pressure brought to bear on the subject. For this reason, it was decided to study separately the effects of a sequence of moderate and the effects of a sequence of extreme trials. Hence, this experiment had two conditions, Moderate and Extreme. Each condition began with two neutral trials to be sure the subjects understood the task; however, the interspersed neutral trials were dropped in order to keep constant the application of pressure, once the critical trials began. The sequence of 38 trials for the Moderate condition was

N, N, M, M, M, ... , M

while for the Extreme condition, the sequence was

N, N, E, E, E, ... , E .

The implications that these differences between the present research and that of Asch have for the development of stochastic models are important to consider. The additional variables built into the Asch procedure could have two kinds of effects: They could systematically alter the response patterns,[8] or they could increase the variability of these patterns. Either of these effects would pose serious problems in the development of a model to describe this situation. For example, in the case of systematic effects, a model that would describe these effects would require several parameters. Data from experiments using Asch's procedures do not offer a large enough number of observations to estimate each of the parameters involved. Similarly, the effect of increased variability would be to make the estimation problem more difficult; the statistics that would be used to estimate parameters would have too much variance to make them very reliable estimators. The present research has attempted to abstract the core of the phenomenon that Asch has studied while holding other factors constant; the intention is first to describe this core and then use this description in an effort to account for other factors. If, for example, a model were found which described the present experiment, it would be possible to test the hypothesis that these other variables had no significant effect on conformity behavior. If that hypothesis were true, we should expect a model with parameter values drawn from the present research to describe the data from Asch's experiments.

To say that the details of Asch's design are not well suited to this investigator's purpose is not to criticize Asch. His main interest has been the demonstration of a phenomenon under a variety of conditions. His results, as well as some of his conceptualization, led to this author's investigation.

Before proceeding to the data, a few points about the study should be mentioned. The research was conducted at a New England college.[9] The subjects were undergraduates recruited from introductory psychology courses and through the student employment services; the former group served as volunteers, while each of the latter was paid one dollar for his services.[10] The confederates were members of a section of the "Communication and Organization" course, for which they could use participation in this study as a term paper topic. The confederates were unpaid.

The practical problems of conducting this experiment led to two sources of variation, the effects of which could not be assessed. First, it was not possible to assign subjects randomly to each of the conditions. The subjects were run at half-hour intervals, and there was not time to arrange for the confederates to switch their responses from one subject to the one immediately following. Rather than risk the confusion that might result from some covert signal to the role players, confusion that did result from some of the confederates missing a cue when such a method was tried, it was decided to complete the Moderate condition before beginning the Extreme. Second, since the confederates were all volunteers, it was not possible to use the same majority for all subjects. In fact, because of scheduling difficulties, no group of confederates served for more than four subjects. This, coupled with the fact that teams of role players did not stay together as units, makes it impossible to analyze the effects of varying the composition of the group of confederates.

Thirty-six subjects were run in the Moderate condition and thirty in the Extreme. Since prior knowledge of the experiment would influence a subject's behavior in this situation, any subjects who had indicated prior knowledge of this particular experiment were excluded from the analysis. It was necessary to eliminate only six subjects, three in each condition, because they knew the purpose of the study. For the analysis, then, there were thirty-three subjects in the Moderate and twenty-seven in the Extreme condition. For reasons that will be clear later we shall use M to denote sample size.[11]

2.4 Preliminary Analysis of the Data

This section will present a few of the salient statistics computed from the data. At this point, the questions of interest are the following. How successful was the attempt to produce conformity under more restrictive and artificial conditions than those of the Asch experiments? How do the data from this research compare with Asch's basic experiment (Experiment 1)? Is there any evidence of temporal trends in either Moderate or Extreme condition? Do these conditions differ? More intensive analyses will await an examination of the models considered in this study; it was the examination of models that suggested some of these further analyses.

Table 2.1 shows the frequency distribution of errors for the Moderate and Extreme conditions. The raw data on which this and the following tables are based will be found in Appendix B. In constructing this table, moderate and extreme responses have been combined as "errors" for the Extreme condition; there were no extreme responses in the Moderate condition.

This table indicates that the procedure was somewhat successful in producing conformity behavior. Although the mode of the distribution in both conditions is complete nonconformity, that is, complete independence of the majority, 70 per cent of the subjects in the Moderate and 78 per cent of the subjects in the Extreme condition made at least one error. In addition, there are several subjects who conformed on more than 80 per cent of the trials. When these data are compared with the results from two control groups that Asch ran, the conformity produced in the present experiment is clearly demonstrated. The subjects in Asch's control groups judged the same stimuli as his experimental subjects but in the absence of a rehearsed majority. Control group A, the control for Asch's Experiment 1, made only 3 errors out of a possible 420 (37 subjects, each making 12 responses), and control group B, the control group for Asch's Experiment 5, made 4 errors out of a possible 396 (11 subjects each giving 36 responses). The proportion of errors was .007 in control group A and .01 in control group B. As can be seen in Table 2.1, the proportions for the present experiment were .299 and .212 for the Moderate and Extreme conditions, respectively.

There is further evidence that the procedure was successful in some of the comments which the subjects made following the experiment. To quote just a few of these comments:

> I began to feel that I was crazy. (Subject who made 25 errors.)
>
> Why did I get different answers? It was funny that six could give one answer and one a different answer. (A completely independent subject.)

Table 2.1. Comparison of Frequency Distribution of Errors for the Moderate and Extreme Conditions

Number of Errors	Moderate (M = 33)	Extreme (M = 27)
0	10	6
1	3	4
2	2	3
3	1	2
4		2
5		
6	1	1
7		2
8		2
9	1	
10	1	
11		
12	1	
13	1	
14		
15	2	
16	1	
17		
18		
19	1	
20		
21	1	1
22		
23	1	
24		
25	1	
26		
27		
28		
29		
30	1	1
31	1	2
32		
33	2	
34	1	1
35		
36		
$\overline{X}$	10.8	7.7
Per Cent of Possible Errors	29.9	21.2

> I wasn't sure of my answers, but I wasn't sure the others were right either. I just didn't have the strength of my convictions. I felt bad when I made choices going with the group; I didn't think I was right then, but I got tired of bucking them. (A subject who made 9 conforming responses.)

> I know I need new glasses. (A completely independent subject.)

In addition to these comments, there were noticeable signs of tension from many of the subjects; hesitation, squinting, and restlessness were common occurrences.

An attempt to compare the magnitude of the majority effect in the two conditions of the present experiment with the magnitude of the effect that Asch obtained poses a slight dilemma. Asch's Experiment 5 is most comparable with the present work; however, the number of subjects in this experiment is too small for reliable comparisons. On the other hand, Asch's Experiment 1, in which there were a large number of subjects, had only 12 critical trials. It would not be appropriate to compare a distribution of errors based on this short sequence with distributions from the present experiment which are based on 36 trials, for any observed differences might be because tendencies to conform increased with more applications of social pressure. To resolve this dilemma, it was decided to use only the first 12 critical trials from each condition.

Table 2.2 presents the frequency and percentage distributions for the Moderate and Extreme conditions and for Asch's Experiment 1. The null hypothesis that each of the present distributions did not differ significantly from the distribution that Asch obtained was tested, using the chi-square test of goodness of fit. For the 5 per cent level of significance, the table shows that this rather stringent null hypothesis cannot be rejected for either condition; for the Moderate condition, chi square equals 18.7, which for twelve degrees of freedom gives a p of .10; for the Extreme condition, chi square equals 10.7, yielding a p greater than .50. The size of the p value for the Moderate condition is not disappointing because the hypothesis tested was more strict than necessary. The only conclusion desired at this point is that these results are similar to those previously obtained, not that they were identical within the limits of sampling. This conclusion is justified by the data.

The next issue to consider concerns temporal effects. Does the proportion of correct responses increase or decrease as the sequence of trials continues? The reader will recall that Asch found little evidence of temporal trends. Contrary to his expectation, there was no tendency for the majority to produce increasing conformity with repeated application of pressure. There are several possible explanations for the absence of

Table 2.2. Comparison of the First Twelve Trials of the Moderate and Extreme Conditions with Asch's Experiment 1

(Frequency and Percentage Distributions)

	Moderate		Extreme		Asch Experiment 1	
	(M = 33)		(M = 27)		(M = 123)	
Number of Errors	Frequency (per cent)		Frequency (per cent)		Frequency (per cent)	
0	12	36.4	10	37.0	29	23.6
1	3	9.1	2	7.4	8	6.5
2	1	3.0	3	11.1	10	8.1
3	4	12.1	2	7.4	17	13.8
4	3	9.1	2	7.4	6	4.9
5	5	15.2	3	11.1	7	5.7
6	1	3.0	0	0.0	7	5.7
7	0	0.0	0	0.0	4	3.2
8	1	3.0	2	7.4	13	10.6
9	0	0.0	0	0.0	6	4.9
10	3	9.1	2	7.4	6	4.9
11	0	0.0	1	3.7	4	3.2
12	0	0.0	0	0.0	6	4.9
χ^2	18.71		10.66			
df	12		12			
p	≈ .10		> .50			

trends that were discussed earlier. Because this issue is central to the present research, it will be analyzed in the next chapter. At present, a gross examination of the data will illustrate what can be inferred from such an over-all picture.

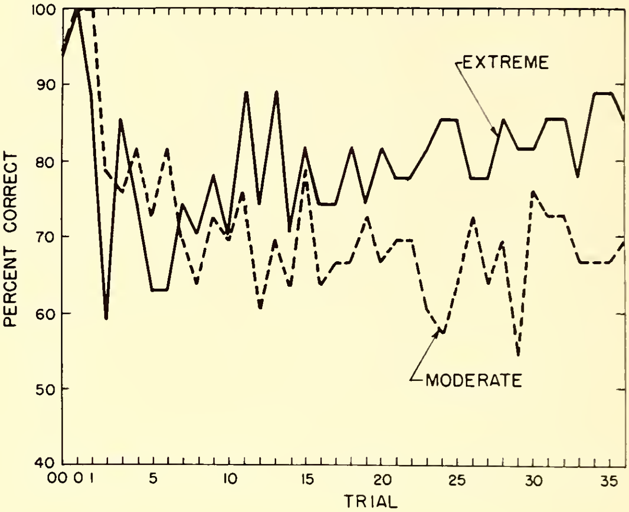


Figure 2.1. Comparison of Moderate and Extreme conditions.

Figure 2.1 presents trends for the Moderate and Extreme conditions. In this graph, percentage correct is plotted against trial. Trials 00 and 0 are the neutral trials, and nearly all subjects were correct on these trials.[12] Beginning with trial 1, the confederates gave incorrect responses for the remainder of the sequence.

Several points should be noted in this graph. First, trial 1, the first critical trial, differs from the succeeding trials in both conditions. In the Moderate condition, no subject conforms to the majority, whereas in the Extreme condition only three subjects respond incorrectly. Second, trials 2 through 6 show sharp differences between conditions. There is marked variability in the Extreme, while there is little in the Moderate; in addition, on all trials but the third, the percentage correct is considerably smaller in the Extreme condition. Third, from trial 7 to the end of the sequence, the Extreme curve shows a consistent trend toward increasing correctness, but the Moderate curve does not show any consistent pattern. To summarize these data, in the Extreme condition the percentage correct is lowest and most variable at the early stage of the experiment, and then increases and becomes less variable, whereas there is no appreciable trend in the Moderate.

Some comments about the differences between the Moderate and the Extreme conditions are pertinent at this point. Before some apparent differences are discussed, a few words of caution are necessary. No tests of significance were applied to these differences because it was felt that the statistics examined were not sufficient[13] to describe the differences between conditions. One purpose of developing a model to describe the process leading to conformity or independence is that such a model would suggest adequate statistics to use in describing the differences between variations of the same process. Because this writer regards behavior in the Moderate and Extreme conditions as manifestations of the same process, he feels that the same model should describe both conditions although the parameter values may differ. If this belief is correct, then these parameter values should be useful statistics in describing the difference between conditions. For the present, however, only some very gross differences can be pointed out, and even these may not be statistically reliable differences.

The most striking difference between the Moderate and the Extreme conditions was shown in Figure 2.1. As was mentioned earlier, the Extreme condition presents a clear and consistent trend toward decreasing conformity, while the Moderate condition shows little over-all trend. In addition to this fact, there is the tendency for the percentage correct to reach a minimum on an early trial in the Extreme condition but to descend more slowly in the Moderate condition. Furthermore, since the minima in both cases are about the same, there is a considerable difference between percentage correct on the early trials. Another point to note is that while the Moderate condition has a slightly higher proportion of errors for the whole sequence of trials, the Extreme condition has a slightly higher proportion of subjects who make at least one error. This is shown in Table 2.1. Some additional differences will be seen in further analyses, but these analyses as well as an interpretation of the differences just discussed must await an examination of the probability models considered in this study.

At this point, it should be mentioned that the special devices included in the stimulus sequence did not systematically affect the subjects' responses. Each subject had 18 trials with the standard to the right and 18 to the left of the comparison lines. The difference between the number of errors on the left and on the right was tested using the Wilcoxon Signed Rank Test,[14] and no significant difference was found in either the Moderate or the Extreme condition. Data for this analysis will be found in Appendix B. Effects due to varying the labels of the comparison lines were examined using Friedman's analysis of variance for ranked data.[15] No significant differences among the seven differently labeled sets of stimuli were found in either experimental condition. These data also are presented in Appendix B. While our efforts to break the monotony of the stimulus sequence and allay suspicion did not introduce systematic effects, it is possible that these variables increased the variability of the subjects' responses. For present purposes, however, it is sufficient that these devices did not introduce systematic effects.

2.5 Summary

This chapter described Asch's experimental procedure and discussed some of his findings. It presented the points at which the present design differed from Asch and also compared data collected in this study with results from Asch's Experiment 1. The analyses of the present study were quite preliminary; more intensive examination of the data will follow discussion of several probability models.

The experimental procedure of the present study may be summarized as follows: A naive subject arrives for a group experiment in visual perception; he is told to match one of three comparison lines to a standard line; unknown to him, the other members of the group have been instructed to give unanimous and incorrect responses on all trials after a two-trial "warm-up"; he is then presented with 38 identical sets of lines, which have varying labels and to which the confederates give 2 correct, followed by 36 incorrect responses. Following the thirty-eighth trial, the experimenter and role players question the subject about his disagreement with the group to determine if he were at all suspicious and to stimulate him to discuss his reaction generally.

This experiment had two conditions, Moderate and Extreme, alike in every respect except the behavior of the confederates. In the former, all of the group's incorrect responses are moderate errors, while in the latter, the confederates' errors are extreme. The difference between conditions is coordinated to a conceptual distinction in amount of social pressure being applied to the naive subject.

There were three important departures from Asch's procedure in the present investigation. These were (1) holding the stimulus lines constant from trial to trial, (2) holding the group responses constant from critical trial to critical trial within a condition, and (3) eliminating the interspersed neutral trials, which we believe reinforced the subjects' tendency to respond correctly (see p. 46).

Some devices designed to ensure the success of the experiment were mentioned. These were varying the position of the standard with respect to the comparison lines, varying the labels of the comparison lines, and beginning the sequence of trials with two neutral trials.

Preliminary analysis of the data demonstrated the following points:

1. The experiment was successful in producing conformity; the amount of conformity was very similar to that which Asch obtained.
2. There was a consistent temporal trend in the Extreme condition and no apparent trend in the Moderate. Hence, there appeared to be differences between conditions.
3. There were no systematic effects because of the devices employed.

NOTES

1. Although we shall use the term "independent" in a statistical sense later, we use it in this discussion in order to follow Asch's terminology. The sense in which independence is meant should be clear from the context.

2. The number of role players in Asch's studies varied from four in one experiment to ten in another.

3. Festinger (1953) distinguished between overt compliance with and without "private acceptance." Overt compliance without private acceptance refers to behavior that is in accord with given norms by an actor who privately rejects these same norms. This distinction corresponds to that between behavior with and without "internalization" of norms. Since Asch reports the phenomenon as a distortion of *response*, we do not believe that the process to be described involves internalization. We note this distinction because it may be that such a process is at least quantitatively different from that of motivating the internalization of norms. When there is internalization, the behavior will occur even when social pressure is absent; since, however, Asch's work always involves the physical presence of the confederates, we can postpone this issue with the disclaimer that our analysis may not apply to situations involving internalization of norms.

4. Complete instructions and a sample set of stimuli are in Appendix A.

5. When the group made a moderate error, no subject ever made an extreme error. On the other hand, when the group made an extreme error, the subject often made a moderate error. In this case, the subject's behavior represented a

compromise between complete yielding to the group and complete independence. When this form of compromise does occur, the category "incorrect," which combines compromise and complete yielding, does distort the data somewhat. For model building, however, this categorization is desirable. Once we have an adequate model, it may be possible to recover the distinction, but such an effort would require additional experimental work and will not be attempted in the present study.

6. Since the stimuli were constant, it was possible to have them printed, thus ensuring that all were identical. Printing, however, ruled out the possibility of randomizing the labels because that would have raised the cost. And to label the stimulus cards by hand would have called attention to the incidental nature of the identifying letters and thus to the constancy of the stimuli.

7. From another point of view, the trials on which the group is correct can be considered trials that reinforce tendencies to give the correct response, that is, tendencies to independence. This conceptualization, of course, would make the term "neutral" inappropriate.

8. For example, the difference between moderate and extreme trials can be interpreted in several ways. Festinger (1950) presents the hypothesis that social pressure increases with the discrepancy between the group and the individual. Applying this hypothesis to the present situation, we should argue that the amount of social pressure increases from moderate to extreme trials. Hence, the amount of conformity should increase from moderate to extreme trials. On the other hand, Festinger (1950) offers an additional hypothesis that would predict a decrease in conformity from moderate to extreme trials. That is, we may infer from his work that the tendency to reject the group increases with the discrepancy between the group and the individual. In any case, a systematic difference is predicted between moderate and extreme trials. To determine the nature of this difference, it is necessary to separate temporal effects from the effects of social pressure. It is one purpose of the present design to do just this.

   Since we have mentioned Festinger's hypotheses, we should comment briefly on them. Using either of these hypotheses singly, one could account for any difference between Moderate and Extreme conditions. However, the hypotheses were intended to be used jointly in determining a resultant amount of social pressure. It is often difficult, nevertheless, to determine the resultant of these conflicting forces. We intend to avoid this difficulty by considering only the conflict aspect of the situation. Chapter 4 will present this interpretation in detail. For the moment, we merely state that we consider the amount of conformity as a function of the degree of conflict and regard the Moderate condition as representing a greater conflict than the Extreme, especially since the possibility of compromise is present in the Extreme.

9. The author is indebted to Drs. A. Paul Hare, Warren Bennis, and William McGill for their aid and cooperation. In addition, he is grateful to the students of Dr. Hare's course for their services as confederates.

10. There were no differences in responses for paid and unpaid subjects.

11. M will denote Moderate trial also but the meaning should be clear from the context. N will be reserved for the number of trials.

12. In the Moderate condition, two subjects erred on trial 00, and in the Extreme, one subject responded incorrectly on trial 00. All subjects in both conditions were correct on trial 0.

13. That is, the statistics computed do not use all the information in the data. If, for example, the difference between the mean number of errors in each condition were tested, the result would not yield any information about the differences revealed in Figure 2.1, which plotted percentage correct against trial. For a detailed discussion of the technical meaning of sufficiency, the reader is referred to Cramer (1946).

14. See Mosteller and Bush (1954) or Walker and Lev (1953).

15. Ibid.

Chapter 3

BASELINE MODELS

3.1 Introduction

This chapter has two main objectives, first to investigate the substantive problems of temporal trends and individual differences, and second, to illustrate the process of developing and testing a model. To accomplish these objectives, the chapter will discuss several "baseline" models that incorporate assumptions relevant to these substantive problems. For the most part, these baseline models make no assumptions regarding the nature of the process but do make statistical assumptions about the nature of the data. In fact, they can be regarded as null hypotheses concerning, for example, whether or not there are any time trends in the data. Three such baseline models will be considered in an effort to demonstrate the application of theorems from probability theory, estimation procedures, and methods of testing the fit of a model to the data. In addition to these baseline models, one of the early substantive models examined in this investigation will be discussed to emphasize the contrast between a model which makes substantive assumptions about the nature of the process and one which does not.

In one sense, the baseline models to be examined ask the question, "Is there any temporal process in these data to describe?" Before considering complex descriptions of a process, it would be well to answer this question conclusively. It would, however, be a mistake to regard the discussion of baseline models solely as an effort to determine whether or not there is any need for a substantive model in this area. Assuming such a need exists, the baseline model can offer suggestions for the type of substantive model needed. Similarly, inadequate substantive models can show fruitful directions for further work. In fact, the baseline models examined in this work led directly to the substantive model to be described in Chapter 4, whereas the final model in the present chapter indicated some of the properties a model would require if it were to describe adequately the data from the present experiments.

Data from both the Moderate and Extreme conditions will be used to test the baseline models. Because the Moderate and Extreme conditions are considered to illustrate essentially the same process, any model that does not describe both conditions will be rejected.

The remainder of this chapter will be concerned with variation in the probability of a correct response. Consider the random variable[1] X_{in} which can take on the values 0 and 1. Let X_{in} be 1 if the i^{th} subject makes a correct response and 0 if he makes an incorrect response on the n^{th} trial. We are then interested in the probability that X_{in} equals 1, symbolically $\Pr\{X_{in} = 1\}$. For some of the discussion, the subscript "i" will be dropped, as the focus will be on the entire sample rather than on an individual subject. The notation should be clear from the context.

3.2 The Constant Probability Model

This is the simplest of all the models considered. It assumes that there is no temporal trend and that there are no individual differences. Let p_{in} equal $\Pr\{X_{in} = 1\}$. Then this model asserts that p_{in} equals k, where k is some constant between 0 and 1. This is, then, a one-parameter model with k as its parameter. The first task is to estimate k. The method of moments will be used; k will be fitted to the mean of the distribution of correct responses. Thus the mean number of errors in the Moderate condition was 10.8, as was seen in Table 2.1, so that the mean number of correct responses was 36 - 10.8 = 25.2.

The theoretical distribution of correct responses is the distribution of $\sum_n X_{in}$, and the expected value of this sum is

$$E\left(\sum_n X_{in}\right) = \sum_n p_{in} = \sum_n k = nk \qquad (3.1)$$

because $E\left(\sum_n X_{in}\right) = \sum_n E(X_{in})$, and $E(X_{in})$ is simply $\Pr\{X_{in} = 1\}$. Fitting the expected value of this distribution to the observed mean, we have, remembering that n is the number of trials (n = 36):

$$n\hat{k} = 25.2, \quad \hat{k} = .70 \text{ for the Moderate condition}$$

and

$$n\hat{k} = 28.1, \quad \hat{k} = .78 \text{ for the Extreme condition}$$

To determine the adequacy of this model, the theoretical variance of the distribution of correct responses can be computed and compared with the observed variances. The expected value of X_{in} and the variance of X_{in} for a single trial are necessary for this computation. But $E(X_{in})$ is $\Pr\{X_{in} = 1\}$ or k, and the variance is given by:

$$\sigma^2_{X_{in}} = E(X^2_{in}) - [E(X_{in})]^2 \qquad (3.2)$$

The value $E(X_{in}^2)$ is also equal to k because it is $\Pr\{X_{in}^2 = 1\}$, and whenever $X_{in} = 1$, X_{in}^2 also equals 1. Therefore,

$$\sigma_{X_{in}}^2 = k - k^2 \tag{3.3}$$

The desired quantity, however, is the variance of $\sum_n X_{in}$. The assumption that the probability of a correct response is a constant from trial to trial enables us to compute this variance. From this assumption it follows that trials are independent; hence, the following theorem that holds when there is independence can be applied:[2]

$$\sigma_{\Sigma X}^2 = \sum \sigma_X^2 \tag{3.4}$$

As the variance of X_{in} is constant for all trials,

$$\sigma_{\sum_n X_{in}}^2 = \sum_n \sigma_{X_{in}}^2 = \sum_n (k - k^2) = nk\,(1 - k) \tag{3.5}$$

The following table compares the theoretical and observed variances of the distribution of correct responses for both the Moderate and Extreme conditions: (The observed value is denoted by s^2.)

	Moderate	Extreme
$\sigma_{\Sigma X}^2$	7.56	6.18
$s_{\Sigma X}^2$	143.38	126.95

It is clear that this model far underestimates the observed variance, and so it is rejected. Whether the large observed variance is caused by individual differences, by the effects of trends, or by both, remains to be determined. The next baseline model attempts to deal with this problem.

3.3 The Individual Differences Model

This model also assumes that there is no trend through time but makes the further assumption that each individual has a different probability of a correct response. That is, to each individual is associated a probability k_i that remains constant throughout the sequence of trials. Formally,

$$p_{in} = k_i, \quad 0 \leq k_i \leq 1, \quad i = 1, 2, \cdots, I$$

This model ~~that~~ then has I parameters, one for each individual.

These parameters could be estimated in much the same way as the single parameter of the model discussed in Section 3.2.

Similarly, the theoretical variance could be computed and compared with the variance actually observed. However, this procedure would require estimating, in the Moderate condition, for example, 33 k's and then computing the variance of each of these k's, summing these variances over trials, and finally summing the sums over individuals. That is, the variance of the distribution of correct responses is given by

$$\sigma^2_{X_{in}} = \sum_i \sum_n (k_i - k_i^2) = \sum_i nk_i (1 - k_i) \qquad (3.6)$$

Evaluating this expression involves the operations just described. This approach is rather laborious and for large samples would be prohibitive. Fortunately, there are approximate techniques.

To test this model, the experimental sequence of 36 critical trials was divided into two sequences of 18 trials each. Trials 1 to 18 constituted sequence 1, and trials 19 to 36 were designated sequence 2. The number of errors each subject made in each half of the experiment[3] was recorded, and a two-way analysis of variance was performed on each condition separately.

In order to meet the assumption of homogeneity of variance necessary for this test, the data were transformed to arc sines, using tables prepared by Mosteller and Youtz (1953). One advantage of using this transformation is that there is a theoretical interaction term against which to test the observed interaction.

Tables 3.1 and 3.2 present the results of these analyses of variance. Subjects are a significant source of variation at beyond the 1 per cent level for both the Moderate and the Extreme conditions. Sequences, however, are significant only for the Extreme condition and at the 5 per cent level. But it is important to note that the observed interaction term is significantly different from the theoretical at beyond the 1 per cent level in both the Moderate and the Extreme conditions. Under the assumptions of this model, the observed interaction terms should not differ significantly from the theoretical; the fact that they are significantly different suggests that the assumptions of this model are inadequate; this result means that for some subjects there are differences between sequences. Thus we can conclude that individuals have different probabilities of responding correctly, but we cannot conclude that all of these probabilities are constants. Furthermore, this finding implies that if in plotting time trends we include those subjects who give correct or incorrect responses on all trials, we may obscure a trend which is present in the data.

Dividing the experimental sequence into two subsequences suggested a further analysis. Plotting the percentage correct against trial for two classes of subjects might resolve the question of

Table 3.1. Analysis of Variance of the Extreme Condition

Source	Sum of Squares	df	Mean Square	F
Total	27,279.4	53	———	———
Subjects	24,329.6	26	935.7	9.82**
Sequences	472.9	1	472.9	4.96*
Interaction + Residual	2,476.9	26	95.3	———

Theoretical Interaction = 44.38

$F_{26,\infty}$ (observed, theoretical) = 2.15**

* Significant at the 5 per cent level.

** Significant at the 1 per cent level.

Table 3.2. Analysis of Variance of the Moderate Condition

Source	Sum of Squares	df	Mean Square	F
Total	40,436.7	65	———	———
Subjects	37,278.8	32	1,165.0	12.21**
Sequences	105.3	1	105.3	1.10
Interaction + Residual	3,052.6	32	95.4	———

Theoretical Interaction = 44.38

$F_{32,\infty}$ (observed, theoretical) = 2.15**

** Significant at the 1 per cent level.

time trend more conclusively. Subjects were classified according to whether they made more errors in sequence 1 or in sequence 2; those who made the same number of errors in both sequences were not included in this analysis. Then the trial-by-trial percentage correct was plotted for each class of subjects.

Figure 3.1 shows the mean curves for these two classes of subjects for the Moderate condition. (This analysis was not carried out for the Extreme condition, as there is little doubt

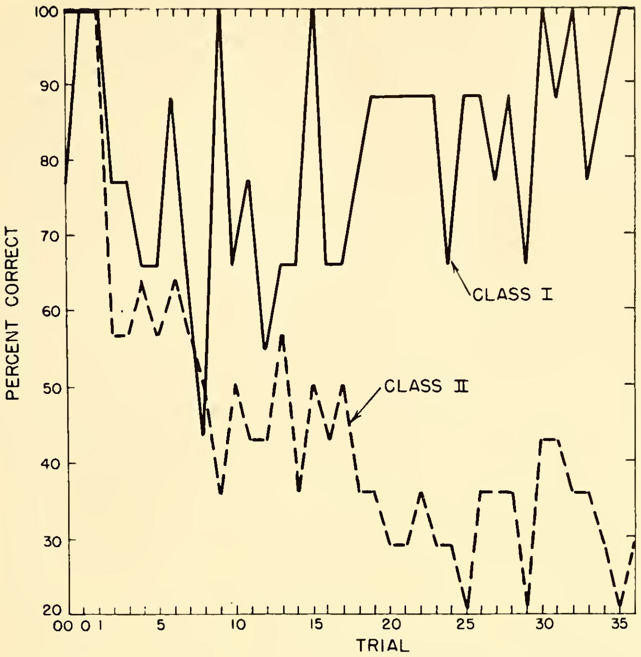


Figure 3.1. Moderate condition: trends of Class I and Class II.

that a time trend exists in the data from that condition.) Class I includes those subjects who had more errors in the first half (sequence 1), while Class II contains those who had more errors in the second half (sequence 2).

Of course, this classification procedure introduces a bias favoring the appearance of time trends. There is bound to be some upward trend for Class I and a downward trend for Class II. But the trend for Class II, shown in Figure 3.1, seems to be much sharper than could be produced by a biased classification. In addition, the almost complete lack of overlap of the curves for the two classes cannot be accounted for by the method of classification. As expected, despite the high variability and the small number of cases, Class I shows some upward trend.

To check how much of the trends in Figure 3.1 was caused by the bias of the classification, a Monte Carlo computation was carried out.[4] First, the probability of a correct response (p_i) was estimated for each subject who fell into either Class I or Class II. (Subjects who made the same number of errors were excluded.) This probability was based on the subject's performance for all 36 trials. Then these probabilities were attached to "stat-rats" (or in the present case, "stat-people") who were run through 36 trials using a random number table. For example, if subject 1 made 12 errors in 36 trials, the estimate of his probability of a correct response was .667. This probability was then attached to "stat-rat" 1, who was run through all 36 trials with this constant p_i. On a given trial, a random number was drawn; if the number was less than p_i, a correct response was recorded, whereas if it was greater than or equal to p_i, an error was recorded. In this way, a set of simulated data was created which could be compared with the observed data.[5] If the assumption that p_i is a constant is correct, our Monte Carlo data should agree closely with the observed data. If the assumption is not correct (and we know this to be the case), then the Monte Carlo data provide information on how much trend can be expected from this biased classification procedure.

The "stat-rats" were classified in the same manner as the subjects, and the percentage correct was plotted for each class. These curves are shown in Figure 3.2. The number of cases in each class of "stat-rats" differs from the number in each class of subjects because the Monte Carlo run led to additional ties.

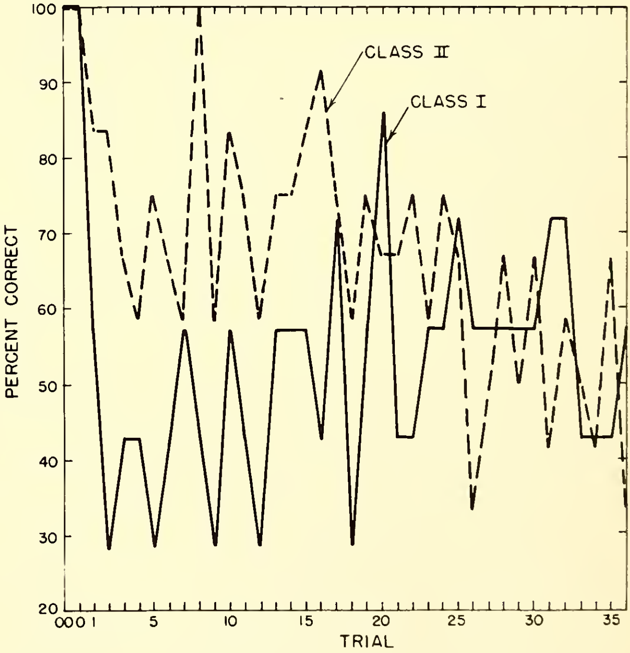


Figure 3.2. Monte Carlo computation of Moderate condition: trends of Class I and Class II.

Some contrasts between Figures 3.1 and 3.2 are worth noting. In the first place, both classes of "stat-rats" show considerably more variability than the classes of subjects. Second, the two classes of "stat-rats" overlap in the second half of the sequence of trials, while the classes of subjects show little overlap. Finally, the trend for Class II of subjects is much more consistent than the trend for the same class of "stat-rats." These contrasts should be viewed conservatively because the samples are small. However, this comparison confirms our previous finding that a model that assumes a constant probability of a correct response is inapplicable, and it points to that group of subjects for whom the model is inappropriate, namely, those subjects in Class II.

A further result of this comparison was the speculation that the experimental data could be explained in terms of four classes of subjects. If the reader recalls that subjects who had the same number of errors in each subsequence were excluded from this analysis, he will readily see the basis for a four-class typology. These four classes are (1) completely independent subjects, (2) completely yielding subjects, (3) subjects who show an increasing tendency to conform, and (4) subjects who show an increasing tendency to independence. This speculation led directly to the model to be discussed in Chapter 4.

3.4 Bush's One-Trial Learning Model

Discussion of this model, which is drawn from Bush (1955), marks a transition between baseline and substantive models. The distinction is not as clear-cut as was implied earlier in this chapter. The assumption basic to this model is that change in behavior is sudden rather than gradual. Although this is a substantive assumption, in the present writer's view, it is a minimal one.

Bush developed this model as a possible formalization of the "noncontiguity" or "insight" position held by some learning theorists. An interpretation of his model in the present situation would be similar to insight learning. That is, each subject responds correctly on all trials with probability p_i until some point when he sees through the subterfuge of the experiment; from that point on, he always responds correctly. The model assumes that in an infinite number of trials all subjects would have this insight, but that the trial on which insight occurs would differ for subjects. In addition to allowing individual differences in the "trial of insight," this model can allow individual differences in the probability of a correct response. Although Bush, in his treatment of the model, assumes that all individuals have the same p, his assumption does not affect an initial test of the model. Therefore, this discussion will follow Bush (1955).

We are again concerned with the random variable X_{in} that is 1 if the i^{th} subject is in some "state" S_0 at the start of the experiment and at the start of some trial N_i changes to another state S_1 for the remainder of the experiment.

Bush then postulates that the probability of a correct response obeys the following law:

$$P_{in} = \begin{cases} p \text{ if the } i^{th} \text{ subject is in state } S_0 \text{ on trial } n \\ 1 \text{ if the } i^{th} \text{ subject is in state } S_1 \text{ on trial } n \end{cases} \qquad (3.7)$$

This one-trial learning axiom can be sketched as in Figure 3.3.

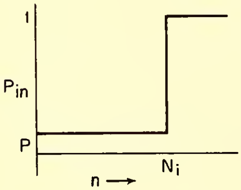


Figure 3.3. Bush's one-trial learning axiom.

Bush next assumes that the change from state S_0 to state S_1, which may be considered the occurrence of event E, occurs with some fixed probability on every trial that the subject is in state S_0, so that

$$\Pr\{i \in S_1 \text{ on } n \mid i \in S_0 \text{ on } n - 1\} = \beta \qquad (3.8)$$

(The left-hand member of this expression should read, "The probability that individual i is in state S_1 on trial n given that he was in state S_0 on trial n - 1.") The probability that trial N_i is a particular trial, say trial j, is given by

$$\Pr\{N_i = j\} = \beta(1 - \beta)^j, \quad j = 0, 1, 2 \cdots \qquad (3.9)$$

According to Bush, these axioms specify a stochastic process that has some simple properties. He suggests that the model can be tested in the following way:

$$\text{Let } q_{in} = \Pr\{X_{in} = 0\}$$

that is, q_{in} is the probability of an incorrect response on trial n, and

$$q_{in} = 1 - p_{in}$$

By assumption, q_{in} has the fixed value[6] q on trials preceding the last incorrect response because event E must occur after the last error. For each subject, we then divide the trials before the last error into two equal parts. (If an odd number of trials are involved, Bush suggests that we eliminate the middle trial.) Let $T_i^{(1)}$ be the number of errors in the first half and $T_i^{(2)}$ be the number of errors in the second half. Then let

$$T^{(1)} = \sum_{i=1}^{I} T_i^{(1)}$$

and

$$T^{(2)} = \sum_{i=1}^{I} T_i^{(2)}$$

According to this model,

$$E(T^{(1)}) = E(T^{(2)}) \qquad (3.10)$$

whereas any model describing a gradual process would predict

$$E(T^{(2)}) < E(T^{(1)}) \qquad (3.11)$$

The null hypothesis of the one-trial learning model, Equation 3. 10, can then be tested using chi square.

This test was applied to the data from the present research, and the results appear in Table 3.3. For the Moderate condition, the null hypothesis can be rejected at beyond the 1 per cent level, but it cannot be rejected for the Extreme condition.[7]

Table 3.3. Number of Errors in Each Ni/2 Trials Before Insight

	Moderate	Extreme
$\sum T^{(1)}$	134	95
$\sum T^{(2)}$	189	86
(Expected)	(161.5)	(90.5)
χ^2	9.36	.35
p	< .01	> .50

Because a model that fits both the Moderate and Extreme conditions is desired, this model was not investigated further. It is interesting, however, to speculate as to the reason this model is not rejected for the Extreme condition. It is quite possible that because the glaring discrepancy between the comparison line which the group chose and the correct answer, subjects became aware of the nature of the experiment and changed their responses accordingly. If such were the case, it is quite reasonable that an "insight" model should describe the phenomenon.

3.5 The Consistency of Group Pressure Model

The consistency model was the first model developed in this research; it was constructed before running the experiment and tested on data from Asch's Experiment 1. The results of analyzing this model influenced the design of the present experiment, particularly the decision to separate moderate and extreme sequences into two experimental conditions. Although a rationale for this design did not come directly from examining this model, in the course of such examination there arose the question of how a "pure" sequence of either moderate or extreme trials would differ from a "mixed" sequence. The experiment was designed in an effort, eventually, to answer that question, and Chapter 8 will consider the problem.

Since this model relates to Asch's rather than to the present experiment, the data to be examined are those of Asch's Experiment 1.

The consistency model takes no account of individual differences; rather it assumes that all individuals have the same experience during the course of the experiment. The analyses in the earlier sections of this chapter suggest that this assumption is inadequate. At the time, however, this fact was not properly appreciated. But it is interesting to see in what respects a model with this obvious shortcoming fails to predict the data.

In contrast to the discussion of the previous models in this chapter, this section will first present the formal model and identify the elements of the model with components of the experimental situation, and only then offer an interpretation of the model. Because this model is more complex than the ones previously considered, it is more expedient to reserve the interpretation until the formal model has been fully discussed.

Let p_n be the subject's probability[8] of a correct response on trial n. We assume that one of three environmental events must occur on a given trial, and we call these events E_1, E_2, and E_3.

In the present experimental situation let us identify these events as follows:

E_1 The confederates give an incorrect response (either moderate or extreme) on trial n after they had given a correct response on trial n - 1.

E_2 The confederates give an incorrect response (either moderate or extreme) on trial n after they had given an incorrect response on trial n - 1.

E_3 The confederates give a correct response on trial n.

A rationale for these identifications will be given shortly. At present, the reader should note that events one and two are defined in terms of a pair of adjacent trials. The first, E_1, concerns the transition from a neutral to a critical trial, and the second, E_2, concerns the transition from a critical trial to a critical trial. We further assume that

$$\left.\begin{array}{l} \text{If } E_1 \text{ occurs, } p_n = O_1 p_{n-1} \\ \text{If } E_2 \text{ occurs, } p_n = O_2 p_{n-1} \\ \text{If } E_3 \text{ occurs, } p_n = 1 \end{array}\right\} \qquad (3.12)$$

where O_1 and O_2 are operators associated with the events E_1 and E_2. An operator is a device indicating that some operation is performed on p_n; the previous expressions do not mean that p_n is to be multiplied by O_1 or O_2 because the form of the operators remains to be specified.[9] Axioms 3.12 incorporate one further assumption that should be explained. The change in p_n from trial n - 1 to trial n depends only on the event occurring on trial n and not on the events occurring on trials before n. Of course, the value of p_n is determined by the previous sequence of events.

Let us now specify the form of the operators:

$$O_1 p_{n-1} = \alpha_1 p_{n-1} \qquad (3.13)$$

$$O_2 p_{n-1} = \alpha_2 p_{n-1} + (1 - \alpha_2)\lambda \qquad (3.14)$$

The form of these operators follows Bush and Mosteller (1955).[10] Once we have specified the initial value of p, the process is determined. Therefore, let

$$p_0 = 1 \qquad (3.15)$$

The basis for this model is a view of the operation of social pressure which holds that consistent application of pressure will have different effects from inconsistent application of the same pressure. First, it should be noted that the events bringing about changes in the probability of a correct response all involve the application of social pressure. Event E_1 can be considered as the inconsistent application of pressure; E_2 can be seen as the consistent application; and E_3 can be viewed as the application of pressure in the same direction as the subject's perception. It is argued that these three types of events will differentially affect the subject's probability. The fact that consistency and inconsistency are considered only for an adjacent pair of trials is a concession to expediency. A model that dealt with a longer time span would be more difficult to handle, and also the fact that there were only 12 critical trials in this experiment makes it impractical to use longer time periods. (If this model proved fruitful, it might then be worth while to incorporate a longer time span when we examine data from the present experiment.)

The parameters α_1 and α_2 can be considered "rates of change" in the probability of a correct response under inconsistent and consistent pressure, respectively, whereas the parameter λ can be regarded as a limit point at which the probability will eventually stabilize if the process is continued for an infinite number of trials.

The model assumes that the probability of a correct response is initially equal to 1; data from Asch's control group approximates this assumption. In addition, it assumes that the probability goes to 1 every time there is a neutral trial, no matter at what point in the sequence of trials this event (E_3) occurs. What this latter assumption means is that whenever a neutral trial occurs, the process, in effect, starts over again. To support this premise, we conjecture that whenever the confederates are correct, they act to reassure the subject that his perception can be correct, and that he has not misunderstood the instructions. This reassurance considerably reinforces the subject's tendency to respond correctly, perhaps by restoring his original self-confidence. Even though this conjecture may be correct, the assumption is probably more restrictive than we should like, but it does represent a good starting point for investigation.

The problems of estimating parameters and computing theoretical values for certain statistics, in order to compare these with the observed values, are not trivial for this model. The method of moments, which was used earlier, becomes quite laborious for a model of this complexity. Similarly, we are no longer in a position to compute the theoretical variance of the distribution of errors for this model because we no longer have independent trials; that is, this model assumes sequential dependencies, so that the variance of a sum is no longer the sum of the variances but rather includes covariance terms. What can be done, however, is to write p_n in terms of the model for each value of n, then by the method of maximum likelihood[11] obtain estimates of the parameters, and finally use these estimates for numerical or Monte Carlo computations. The method of maximum likelihood will not be discussed in detail here, but the principle is essentially one of finding those values of the parameters that maximize the probability of obtaining the observed sample, given that we can write the quantities to be observed in terms of the parameters. In the present case, these are the probabilities of a correct response on each of the 12 critical trials and each of the 6 neutral trials. Remembering from Chapter 2 that Asch's Experiment 1 had the following sequence of trials:

N, N, M, M, N, E, M, E, E, N, N, M, M, N, E, M, E, E

labeling the trial before the start of the experiment as trial 0, we can write:[12]

$$\left.\begin{array}{ll}
 & \text{from Equation} \\
p_0 = 1 & 3.15 \\
p_1 = 1 & 3.12 \\
p_2 = 1 & 3.12 \\
p_3 = a_1 & 3.13 \\
p_4 = a_2a_1 + (1 - a_2)\lambda & 3.14\text{, remembering } p_{n-1} = p_3 = a_1 \\
p_5 = 1 & \\
p_6 = a_1 & \\
p_7 = a_2a_1 + (1 - a_2)\lambda & \\
p_8 = a_2[a_2a_1 + (1 - a_2)\lambda] + (1 - a_2)\lambda & \\
\quad = a_2^2a_1 + a_2(1 - a_2)\lambda + (1 - a_2)\lambda & \\
p_9 = a_2[a_2^2a_1 + a_2(1 - a_2)\lambda + (1 - a_2)\lambda] + (1 - a_2)\lambda & \\
\quad = a_2^3a_1 + a_2^2(1 - a_2)\lambda + a_2(1 - a_2)\lambda + (1 - a_2)\lambda &
\end{array}\right\} \quad (3.16)$$

The final nine trials are exactly the same as the preceding. In examining the set of Equations 3.16, the reader will note some of the difficulties involved in the estimation of parameters. To use the method of moments, it would be necessary to form a sum of the expressions in Equations 3.16, multiply this by 2, since Equations 3.16 deal only with the first 9 trials, and then equate the result to the observed number of correct responses. In addition, because there are three unknown parameters, we must be able to derive two other independent equations in order to obtain a solution. Unfortunately, no direct way to accomplish this was found. As was mentioned earlier, the principle of maximum likelihood offered an alternative approach.

Using Equations 3.16 and the observed proportions correct on each trial, the following estimates were obtained:

$a_1 = .72$

$\lambda = .6$

$a_2 = -.6$

These estimates were then used to compute a theoretical curve of percentage correct against trial. Figure 3.4 compares this curve with the curve observed in Asch's Experiment 1. As the figure shows, these estimates enabled us to fit Asch's mean curve reasonably well. The test of this model, however, is how well it predicts other statistics drawn from the data. To make this comparison, we ran 33 Monte Carlo subjects ("stat-rats")

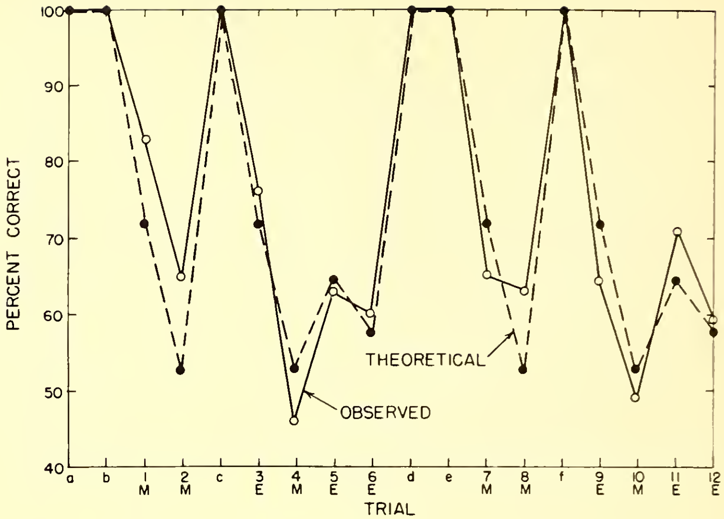


Figure 3.4. Consistency of group pressure model compared with data from Asch's Experiment 1.

Table 3.4. Comparison of Distribution of Errors from Monte Carlo Computation of Consistency Model with that from Asch's Experiment 1

Number of Errors	Monte Carlo		Asch's Experiment 1	
	Frequency	Per Cent	Frequency	Per Cent
0	0	0.0	29	23.6
1	2	6.1	8	6.5
2	3	9.1	10	8.1
3	4	12.1	17	13.8
4	6	18.2	6	4.9
5	9	27.3	7	5.7
6	6	18.2	7	5.7
7	1	3.0	4	3.2
8	1	3.0	13	10.6
9	1	3.0	6	4.9
10	0	0.0	6	4.9
11	0	0.0	4	3.2
12	0	0.0	6	4.9

through 12 trials and tabulated the distribution of errors. Table 3.4 compares this distribution with the distribution of errors from Asch's subjects. Clearly, the two distributions differ. The model predicts a bell-shaped distribution with no density at 0 errors whereas the mode of the observed distribution is near 0. Because this model led to a distribution that departed radically from that actually observed, it was not investigated further.[13]

The discrepancy between the distribution of errors arising from the consistency model and the observed distribution suggests some of the properties an adequate model should incorporate. A model which assumes that all subjects have the same probability of a correct response and that this probability changes in the same way for all subjects leads to a bell-shaped distribution; therefore, a model which would give rise to a skewed distribution with a mode near 0 should allow for individual differences in experience and should assume that social pressure has differential effects. Furthermore, it should allow the possibility that this pressure had no effect for a large portion of the subjects.

3.6 Summary

This chapter has examined several models that included assumptions about the absence of time trends, individual differences, and the variations in social pressure. In addition, a model which assumed that subjects acquired "insight" about the experimental situation was considered.

From the analysis, we concluded that there were time trends for at least one group of subjects, that individuals differed in their experimental experience, and that insight into the experimental situation could account for the data from the Extreme condition. But all the models examined were rejected for at least one of the experimental conditions.

This chapter suggested that an adequate model should allow for individual differences in experience in the experimental situation, although it should be pointed out that this fact does not mean that we must assume individual differences at the start of the experiment.

In pursuing these baseline models, this chapter illustrated some of the procedures for estimating parameters and for testing the fit of a model to the data.

NOTES

1. See Feller (1950) for a discussion of random variables and probability theory.
2. It has been pointed out to the author that the assumptions at

the beginning of this section are not sufficient to guarantee independence. What we are assuming in this model is that we have independent binomial observations.

3. This analysis could be done by dividing the sequence into more than two subsequences. In fact, more subdivisions would undoubtedly allow a more sensitive test for trends. For the present purpose, however, the crude subdivision is adequate; if sequences are a significant source of variance, one could argue that there is at least some trend in the data.

4. Monte Carlo computations are one method of obtaining approximate values for such quantities as means and variances of distributions that arise from the operation of a probability process. Monte Carlo methods involve the use of random numbers to generate a set of simulated data by purely mechanical means. The techniques we use carry a hypothetical subject through a sequence of experimental trials by comparing the assumed probability of a correct response for that subject with a random number and deciding whether or not the subject made the correct response on each trial. The procedure continues until we have generated a set of simulated data to compare with the data we actually collected. These hypothetical subjects have been termed "stat-rats" because the method has been frequently used to simulate animal learning experiments. For a fuller discussion of Monte Carlo methods, see Bush and Mosteller (1955), pp. 128 ff.

5. An alternative procedure that F. Mosteller suggested to the author is to use stat-rats with a guaranteed number of errors, and randomly permute errors and correct responses. This would reproduce the desired classes more adequately since it would reduce the number of ties.

6. $q = 1 - p$.

7. The initial assumption of this investigation is that the Moderate and Extreme conditions are manifestations of the same process and hence are similar. Examination of the models in this study is, in part, an effort to test this assumption.

8. The absence of the subscript i indicates that this probability applies to all individuals.

9. For a discussion of operators, see Bush and Mosteller (1955), p. 18.

10. Especially pp. 27 ff.

11. See Hoel (1947).

12. In this usage, trial 0 refers to the state of affairs before the beginning of the experiment. Later on, we shall consider trial 00 and trial 0 as the initial two neutral trials before the start of the process of influence.

13. No further properties of this model were investigated because we regard the mean curve and the distribution of errors as the most important properties for the model to predict. This rough criterion will apply to the investigation of the remaining models.

Chapter 4

THE CONFLICT MODEL

4.1 Introduction

The previous chapter suggested that an adequate model should allow for individual differences in behavior during the course of the experiment. This does not mean that we must assume individual differences prior to trial 1 of the experiment. Differing response sequences could also be accounted for by a process that assumes all subjects are initially alike but arrive at different response sequences in some probabilistic manner. The model to be described in this chapter takes the latter approach.

The intention of this research is to develop a model that describes the process involved when specific experimental procedures are used. As the reader will see, our approach enables us to predict summary statistics based on the entire sample of subjects. If, however, we wished to predict the behavior of specific individuals, another mode of analysis might be more suitable. However, one should not rule out the possibility of using the model, together with stratifying subjects on the basis of some personality dimension, to predict the responses of specified types of subjects.

The following section describes the general model; the remainder of this chapter suggests a possible social-psychological interpretation of it and discusses some of the assumptions this model incorporates. In addition, we shall present a test of one consequence of the model which indicated that it was fruitful to investigate the conflict model in more detail. Chapters 5 and 6 will deal with special cases of the model, but we shall return to the general case in Chapter 7.

4.2 The General Conflict Model

The trial-to-trial change in the probability of a correct response will be described by a four-state Markov process that is defined as follows: On a given trial, an individual can be in any one of four states; on the next trial, he can move to another state with a specified probability. These probabilities of movement are called transition probabilities. Once the initial distribution of individuals among the states and the transition probabilities are specified, the process is completely determined.

As before, let

$$X_{in} = \left\{ \begin{array}{l} 1 \text{ if the } i^{th} \text{ subject gives a correct response} \\ \qquad\qquad \text{on trial } n \\ 0 \text{ if the } i^{th} \text{ subject makes either a moderate} \\ \qquad\qquad \text{or an extreme error on trial } n \end{array} \right\} \quad (4.1)$$

and let

$$Y_{in} = \left\{ \begin{array}{l} 1 \text{ if the } i^{th} \text{ subject is in State 1 on trial } n \\ 2 \text{ if the } i^{th} \text{ subject is in State 2 on trial } n \\ 3 \text{ if the } i^{th} \text{ subject is in State 3 on trial } n \\ 4 \text{ if the } i^{th} \text{ subject is in State 4 on trial } n \end{array} \right.$$

The four states of the model are defined as follows:

S_1	If the subject is in this state, he will respond correctly on trial n and on every subsequent trial.
S_2	If the subject is in this state, he will respond correctly on trial n and may or may not respond correctly on subsequent trials.
S_3	If the subject is in this state, he will give~~s~~ an incorrect response on trial n, and may or may not respond incorrectly on subsequent trials.
S_4	If the subject is in this state, he will respond incorrectly on trial n and incorrectly on every subsequent trial.

These states are defined in such a way that if we know a subject's state on trial n, we shall know his response. That is,

$$X_{in} = \left\{ \begin{array}{l} 1 \text{ if } Y_{in} = 1 \text{ or } 2 \\ 0 \text{ if } Y_{in} = 3 \text{ or } 4 \end{array} \right\} \qquad (4.2)$$

This expression enables us to translate the model's predictions into observables and will be important in both the derivations and the Monte Carlo computations. It should be noted, however, that even if we know the subject's response on trial n and all subsequent trials, we do not know the state in which that subject was on trial n. When a subject shows a sequence of correct responses, he could have been in S_2 on n and moved to S_1 on some later trial, and there is even some nonzero probability that he was in S_2 for the entire sequence of trials. Thus, the states are conceptual entities.[1]

Following this definition, we call states S_1 and S_4, "absorbing" states. Once a subject enters either of these states, he is "absorbed," that is, his behavior is determined for the remainder of the sequence of trials. On the other hand, S_2 and S_3 are "temporary" states because being in either of these two states on trial n determines the subject's response on only that trial; his future behavior is a probability process.

Restating the definition of these four states in a slightly different form has some consequences for the trial-by-trial changes in the probability of a correct response, which we are interested in describing. We are now concerned with the conditional probability that the individual i will respond correctly on trial n given that he is in state j. These conditional probabilities are:

$$\Pr\{X_{in} = 1 \mid S_1 \text{ on } n\} = 1$$

$$\Pr\{X_{in} = 1 \mid S_2 \text{ on } n\} = 1$$

$$\Pr\{X_{in} = 1 \mid S_3 \text{ on } n\} = 0$$

$$\Pr\{X_{in} = 1 \mid S_4 \text{ on } n\} = 0$$

Let

$$p_j(n) = \Pr\{S_j \text{ on } n\}, \quad j = 1, 2, 3, 4 \qquad (4.3)$$

and

$$p(n) = \Pr\{X_{in} = 1\}$$

Then

$$p(n) = p_1(n) + p_2(n) \qquad (4.4)$$

The i^{th} subject's probability of a correct response on trial n is the probability that he is either in S_1 or S_2. (As these are mutually exclusive states) the probability that he is in either S_1 or S_2 is simply the sum of the separate probabilities.

why?

To determine the probability of a correct response, then, it is necessary to determine the probability of being in states 1 or 2. First we assume that the probability of being in state j on a given trial, say trial n + 1, depends only on the state in which the subject was on the preceding trial, trial n. Next we assume a set of transition probabilities; for example, the probability of moving from S_2 on trial n to S_1 on trial n + 1 is such a transition probability. We further assume that not all of the 16 possible transitions are possible; more specifically, 8 of the possible transitions have 0 probability. The allowable transitions and their probabilities are defined in Table 4.1.

Table 4.1. Allowable Transitions and Their Probabilities

State on Trial n	Possible State on Trial n + 1	Probability
S_1	S_1	1.00
S_2	S_1	α
	S_2	$1 - \alpha - \beta$
	S_3	β
S_3	S_2	γ
	S_3	$1 - \gamma - \epsilon$
	S_4	ϵ
S_4	S_4	1.00

That a subject in S_1 or S_4 must remain only in S_1 or S_4 expresses the fact that these are absorbing states. It should also be noted that we assume a subject in S_2 can move only to S_1, S_2, or S_3, while a subject in S_3 can move only to S_2, S_3, or S_4. The transition probabilities can be considered conditional probabilities; for example, the second line of Table 4.1 can be written:

$$\Pr\{S_1 \text{ on } n+1 \mid S_2 \text{ on } n\} = \alpha$$

The transition probabilities are arrived at in the following way. A subject can be in one and only one state on trial n. Given that state, he has either one or three possible "moves." These "moves" are all mutually exclusive and exhaustive; hence the sum of their probabilities must equal 1. If there is only one possible "move," it is obvious that it has probability 1. If there are three possible "moves," and we assume that the probability of one of these is α, and the probability of another is β, then the probability of the remaining move is $1 - \alpha - \beta$.

The parameters of this model, then, are those transition probabilities whose values must be estimated from the data. The general model has four parameters: α, β, γ, and ϵ. In considering special cases of this model, we shall equate some of these parameters. Before considering these special cases, however, let us consider what information about the model we already have.

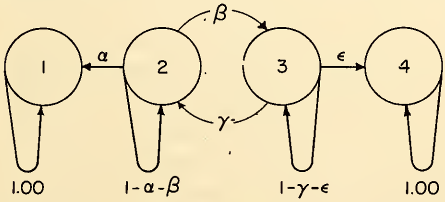


Figure 4.1. States and transitions for the conflict model.

The assumptions made thus far can be sketched schematically in Figure 4.1. This diagram shows the four states, the paths of transition, and the transition probabilities. From these assumptions we can express the probability of being in a given state on trial n + 1 in terms of the states on trial n. We can write, for example,

$$p_1(n+1) = p_1(n) + \alpha p_2(n) \tag{4.5}$$

which simply asserts that a subject's probability of being in S_1 on trial n + 1 is the probability that he was in S_1 on trial n plus the probability that he was in S_2 on trial n and then moved to S_1. It should be noted that if he were in S_1 on trial n, he stays there on trial n + 1, and if he were in S_2, he has probability α of moving to S_1; furthermore, there is no way a subject could be in S_1 on trial n + 1 other than remaining in the state from the previous trial or moving to S_1 from S_2.

Equation 4.5 is called a transition equation. The three remaining equations arise in the same way as Equation 4.5, that is, as we described in the preceding paragraph. Thus, the set of transition equations for this model are

$$\left.\begin{aligned}
p_1(n+1) &= p_1(n) + \alpha p_2(n) \\
p_2(n+1) &= (1 - \alpha - \beta)p_2(n) + \gamma p_3(n) \\
p_3(n+1) &= (1 - \gamma - \epsilon)p_3(n) + \beta p_2(n) \\
p_4(n+1) &= p_4(n) + \epsilon p_3(n)
\end{aligned}\right\} \tag{4.6}$$

In order to estimate the parameters of this model, it is necessary to obtain a solution to this set of linear recurrence relations. Although in principle, there is a solution for the general case, it is not practically feasible and will not be attempted. It is, however, possible to solve a similar equation system for the one- and two-parameter models, and these solutions will be shown in Chapter 5.

Writing these transition equations as in Equation 4.6 illustrates one additional feature of the model. This set of equations requires some information about the initial probabilities of being in each of the states. Considering trial 0 as the trial prior to the beginning of the process — in effect, the second neutral trial in the present experiment[2] — we need $p_1(0)$, $p_2(0)$, $p_3(0)$, and $p_4(0)$. These quantities could be considered additional parameters to be estimated from the data, or we could assume these initial values. Because estimating these from the data would increase the complexity of the model, we have chosen to postulate this initial distribution. The final assumption of the model, then, is that all subjects are in state S_2 at the start of the sequence of trials. Formally:

$$\left.\begin{aligned} p_1(0) &= 0 \\ p_2(0) &= 1.00 \\ p_3(0) &= 0 \\ p_4(0) &= 0 \end{aligned}\right\} \qquad (4.7)$$

This assumption is consistent with both the data and the interpretation of the model we wish to make.

4.3 Interpretation of the Model

The essential feature of the model is that it assumes that the process occurring in this experiment is a process that leads to either consistent conforming or consistent nonconforming. This feature suggests a social-psychological interpretation. Although the rationale is quite tentative, it does indicate further areas of application for the model. It should be emphasized that we are not drawing on any particular theory for this interpretation; theories of conformity are not sufficiently explicit for one to formalize. The assumptions of the model, however, are based on the thinking of many who have studied the Asch situation.

As the name "conflict model" implies, the basis for interpretation is a two-pole conflict situation. The poles of this conflict may be termed "maintaining status in the group" and "maintaining personal integrity." The individual, on the one hand, desires to ensure his status with the confederates, who are his peers. He can do so by conforming to the norm that these peers establish. That the confederates are seen as sanctioning agents and as the source of social pressure is documented by postexperimental interviews both in Asch's work and in the present study.[3] On the other hand, the subject feels some pressure to make his response consistent with internal perceptual standards, to "call them as he sees them." The conflict between this internal pressure and the social pressure applied by the confederates represents the basic element of this experimental situation.[4]

(From one viewpoint, correct responses can also be regarded as conformity; the individual, when he responds correctly, conforms not only to an internalized standard but also to an external norm. It can be argued that the experimenter acts as an external enforcing agent for the correct response. Indeed, some subjects regard the experimenter as a sanctioning rather than a neutral individual, as is best illustrated by the comment, "I didn't go along with them because I was afraid I'd ruin your experiment." It does seem, however, that more than complying to the experimenter is involved in the correct response. Having noted this possible interpretation of correct responses, we shall continue to speak of "correct" and "conforming" or "incorrect" responses.)

We view the social process involved in these experiments as a process of resolving the conflict between the tendency to respond to internal pressure and the tendency to yield to social pressure. The temporary states S_2 and S_3 represent "conflict states," whereas the absorbing states S_1 and S_4 can be considered "resolution states." In addition, we distinguish between S_2 and S_3 according to whether the pressure to maintain one's integrity or the pressure to maintain one's status is dominant at the time. We reason that if the internal pressure predominates, the subject will respond correctly and thus will be in S_2 on that particular trial. On the other hand, if the social pressure predominates, the subject will respond incorrectly and thus will be in S_3. Similarly, we believe that the conflict eventually must be resolved so that the subject consistently responds to only one of these two pressures. States S_1 and S_4 indicate resolution on the side of maintaining one's integrity and maintaining one's status, respectively. Once this conflict is resolved, it remains resolved; that is, one of the two pressures on the subject becomes dominant and remains dominant for the remainder of the sequence of trials. When and at which pole resolution takes place is determined by a probability process.

Each state, then, refers to a different balance of forces. It is felt that the forces are nearly equal in S_2 and S_3 and subject to change, whereas in S_1 and S_4 some threshold has been passed, so that change is impossible. Considering each state in terms of the resultant of the balance of forces leads to a possible interpretation for the parameters. We assume that the forces are such that if an individual is in S_1, he has "rejected" the group; in S_2, he is "leaning away" from the group; in S_3, he is "leaning toward" the group; and in S_4, he has "completely accepted" the group. We then make the following identifications for the parameters:

α	The probability that an individual will reject the group.
β	The probability that an individual will change his leanings toward the group.
γ	The probability that an individual will change his leanings away from the group.
ϵ	The probability that an individual will completely accept the group.

One consequence of this interpretation is that it enables us to use this model and the Asch situation to test two of Festinger's hypotheses concerning "Informal Social Communication" (Festinger, 1950.) Chapter 8 will present this test on the assumption that α and β are functions of the amount of social pressure operating.[5]

The parameters are probabilities of movement between states from trial n to trial n + 1, for all values of n. That is, we assume the transition probabilities are constant through time. This model attempts to describe the situation created by specific experimental procedures where the investigator made an effort to minimize variation in time of crucial elements of the experiment. The subject judged the same lines on each trial, and the confederates responded in the same way on all trials. Furthermore, it is reasonable to assume that the subject's internal perceptual standards did not change from trial n to trial n + 1. The assumption of constant transition probabilities reflects these constant features. Implicit in this assumption, however, is the view that the confederates' response on any given trial constitutes the total social pressure operating on the subject at that time. That is, social pressure does not build up through time. On *a priori* grounds, it is difficult to argue for, or against, the notion of noncumulative social pressure in the Asch situation. The simplicity it introduces into the model, therefore, is its major justification.

The model further assumes that the same transition probabilities apply to all individuals. This axiom incorporates the view that individual differences prior to the outset of the experiment do not affect behavior in the situation. It is here that we run counter to most interpretations of this type of experiment. The evidence, however, is inconclusive. Some investigators have found relationships between measures of personality variables and the frequency of yielding. Factors such as "authoritarianism" as measured by the "F-scale" (Siegel, 1956, p. 228), "need achievement" (McClelland *et al.*, 1953, p. 287) and "status striving" (Siegel, 1956, p. 210) are correlated with conformity in the Asch situation. Since such measures are considered to tap enduring individual characteristics, these findings are inconsistent with our assumption. It should be noted, however, that these reported correlations were not sufficiently high to indicate much predictive power for the trait measures. Furthermore, the present author failed to replicate these findings using two separate samples.[6] In one of these studies, the relationship between status striving and conformity approximated that which Siegel reports (Siegel, 1956), but in the second sample, the correlation was negative. In addition, the present author examined several other trait indices,[7] none of which related to conformity. Finally, Moeller and Applezweig (1957) report that they could distinguish high and low yielders on the basis of the "social approval" scale of their Behavior Interpretation Inventory (Moeller and Applezweig, 1956). Their discussion of this finding is relevant to the present problem:

> . . . It should be emphasized that our prediction was not based on categorization of persons as conformers and nonconformers. . . . We assume that conformity per se is not a trans-situational trait, but that any individual conforms or fails to conform in a given situation depending on his motivational profile and the relevance of conformity behavior in that situation to his motives.[8]

The present author agrees with Moeller and Applezweig in their belief that conformity is not trans-situational and in their emphasis on the motivational profile rather than the operation of a single motive or trait. The assumption we make in this model carries this rationale a bit further. We argue that classifying subjects on the basis of a single trait will not improve the prediction of conformity, because in each individual's constellation of motives and traits, there are some elements to which conformity in this situation is relevant. We assume that these elements differ for different individuals, but the fact that some element is present in every individual indicates that the probability of conforming does not vary greatly from person to person. In other terms, we feel that people conform or do not conform for a variety of reasons, but that some of these reasons are present in everyone. Since we cannot specify in detail the motivational constellation of each individual and since we feel the probabilities have small interperson variability, we consider the assumption that the same parameter values apply to all individuals a reasonable one.

Although the assumption that all individuals are alike prior to the outset of the experiment is a central feature of the present study, it would be possible to modify the model to incorporate individual differences. An alternative model would assert that the parameter values vary as a function of some important personality characteristics. In social-psychological terms, this is equivalent to saying that people vary in the strength of their reactions to social pressure. The procedure for examining this alternative would be to divide the sample into k classes of subjects, each class having a distinct pattern of personality characteristics, and then to estimate the parameters separately for each class. The author is currently pursuing this line of investigation.

When we assume that all subjects are in S_2 at the beginning of the experiment, we also rule out individual differences. This assumption has a situational interpretation, hence we should not modify it to take account of personality factors. The rationale for this assumption can best be seen by examining the alternatives. Since very few subjects conform on the first trial, by definition of States 3 and 4, very few subjects are in those states. The only remaining possibility is that some subjects are in S_1, but this is not testable, for it is not possible to distinguish operationally between States 1 and 2. Even if these two states could be distinguished, assuming that some subjects are in State 1 would be too severe a restriction. It would be tantamount to saying that there are subjects who would not conform in this type of situation, no matter how much pressure they faced.

The assumption that all subjects start in S_2 is an attempt to incorporate the initial reactions of disbelief and resistance to the confederates when these confederates depart from the subject's expectations. That is, it represents the presence of internal perceptual standards. In a situation where both, or neither, alternative behaviors were governed by conflicting internal norms, we should assume that half of the subjects began in S_2 and half began in S_3. Similarly, if we could devise a nontrivial situation where the confederates represented an internal standard and there was an external norm, we should assume that all the subjects began in S_3. The trivial case is illustrated by an experimental sequence of neutral trials where we should expect a 0 probability of transition from S_3 to S_2. (It should be pointed out that the example reinterprets the conceptual states, defining S_3 and S_4 in terms of conformity and S_1 and S_2 in terms of nonconformity, rather than identifying the states with correct or incorrect responses.) Chapter 8 will discuss some situations where these alternative assumptions might apply.

We have offered a tentative interpretation of this model. The interpretation is not unique nor would we argue that it is the most useful interpretation. However, it has aided the development and testing of the model and has the advantage of being related to previous research on conflict and conformity.[9]

To determine whether or not it would be fruitful to investigate this model in detail, we made an initial test of one of its properties. The model assumes that a "process of absorption" is occurring, with some subjects becoming consistently correct and others consistently incorrect. Once a subject is absorbed, he will have only a single run of responses, where a run is defined as a subsequence of identical responses. On the other hand, before a subject is absorbed, he can move about between states S_2 and S_3 and therefore can have a number of runs. Because subjects are more likely to be absorbed the longer the sequence continues, we would expect most subjects to have fewer runs in the second half of the experiment than in the first half. We thus tabulated the number of runs and classified each subject according to whether he had more of his runs in the first or second half of the experiment. This tabulation was made for each condition. The subjects who had only one run for the entire experiment were not included. The results were:

Number of Subjects Who:

	had more runs in first half	had same number of runs in both halves	had more runs in second half
Moderate	18	2	3
Extreme	14	1	6

The null hypothesis that the number of subjects who had more runs in the first half was equal to the number that had more runs in the second half was tested using a sign test. The two conditions were combined for this test, and the null hypothesis was rejected at beyond the 1 per cent level. The result of this analysis, therefore, is consistent with a model that assumes an absorbing process.

4.4 Summary

Thus far we have described a general model, offered a tentative psychological interpretation of the model, discussed the substantive assumptions, and demonstrated that it is fruitful to investigate further this type of model. The next two chapters . pursue the detailed consideration of three special cases of this model. In Chapter 7, we will return to the general conflict model.

NOTES

1. Later in this work, we shall need to estimate the proportion of subjects in S_1 or S_4 on a given trial. We shall use the subject's responses to accomplish this. Such a technique, however, is only a very rough approximation.
2. The present model intends to describe critical trials only. Hence the neutral trials will not be considered except as the state of the system prior to the start of the process. In the figures to be presented in the remainder of this monograph, the first neutral trial will be labeled 00 and the second neutral trial will be labeled 0.
3. See Chapter 2, p. 26, and Asch (1956).
4. We use the terms "internal" and "social" to distinguish the source of pressure. We do not intend to imply that one operates internally and the other externally.

5. Chapter 8 will assume that both α and β are monotonic functions of social pressure. What follows in the present discussion assumes merely that changes in social pressure will produce changes in the parameters.

6. There could be several reasons for this failure to replicate. One possibility which should be recognized is that the previous findings were obtained using Asch's Experiment 1, so that the lack of relationship in the present author's data may be owing to the differences between his procedures and Asch's. If, however, previous findings are of so little generality that changes in procedure affect replication, there is additional support for the position taken in this research.

7. Among these indices were: Two scales from the "Minnesota Multiphasic Personality Inventory," the "Value Profile" of Bales and associates, the "v-achievement" scale of McClelland, and the "Fundamental Interpersonal Relationships Orientation" of Schutz. None of these instruments showed reliable relationships to conformity.

8. Moeller and Applezweig (1957).

9. See Festinger (1950), for example.

Chapter 5

THE ONE- AND TWO-PARAMETER MODELS

5.1 Introduction

This chapter considers the two special cases of the conflict model where most of the properties of interest can be derived analytically. The general model and the three-parameter case require us to use approximate methods to obtain many of the relevant properties. To simplify exposition and to indicate the heuristic value of examining the most restrictive, and mathematically simplest, cases, we have chosen to examine the one- and two-parameter cases first. Approaching the analysis in this manner would contribute to our understanding of the model before we attempted to deal with the complexities of the general case.

In the following sections, then, we shall consider estimation of the parameters and derivation of theoretical values for statistics such as the probability of a completely correct sequence of responses (that is, the probability that a subject never conforms throughout the experiment), the asymptotic probability of a correct response, and the variance of the distribution of errors. Section 5.2 examines in detail the one-parameter case, while Section 5.3 analyzes the two-parameter case only insofar as it is necessary to show that it does not provide any improvement in its fit to the data over the single-parameter model. Throughout this chapter, we first derive the property under consideration; then we compute the relevant statistic from the data, and finally compare expected and observed values.

5.2 The One-Parameter Model

In addition to the assumptions made in Section 4.2, this model assumes that the transitions between adjacent states are equally probable; for example, a subject in S_2 is equally likely to move to S_1 or S_3 on the next trial. We assume:

$$\alpha = \beta = \gamma = \epsilon$$

and thus can rewrite Equations 4.6:

$$\left.\begin{aligned} p_1(n+1) &= p_1(n) + \alpha p_2(n) \\ p_2(n+1) &= (1-2\alpha)p_2(n) + \alpha p_3(n) \\ p_3(n+1) &= (1-2\alpha)p_3(n) + \alpha p_2(n) \\ p_4(n+1) &= p_4(n) + \alpha p_3(n) \end{aligned}\right\} \qquad (5.1)$$

To estimate α, we shall follow the procedure used in Chapter 3.[1] That is, we shall find $E(\sum X_n)$ in terms of the parameter α, equate this to the observed mean of the distribution of correct responses and solve for α. In addition to estimating α, we shall derive from the model the asymptotic probability of a correct response, the probability that a subject will respond correctly on all trials, and the mean and variance of the distribution of alternations.[2] Each derivation will yield a theoretical value of the quantity under consideration which will be compared with the observed value drawn from the data for the Moderate condition. Finally we shall run a number of Monte Carlo subjects and compare additional statistics computed from the Monte Carlo sample with the data from the Moderate condition.

To find $E(\sum X_n)$, we require an expression for $E(X_n)$ that can then be summed.[3] But this is simply the probability of a correct response on trial n. From Equation 4.4,

$$E(X_n) = p_1(n) + p_2(n) = p(n)$$

Hence, it is necessary to find an explicit expression for p(n). We shall now show that $p(n) = 2/3 + 1/3\,(1-3\alpha)^n$. Let

$$\delta(n) = p_2(n) - p_3(n) \qquad (5.2)$$

then from Equations 5.1, subtracting the third from the second equation gives

$$\begin{aligned} \delta(n+1) &= (1-2\alpha)p_2(n) + \alpha p_3(n) - (1-2\alpha)p_3(n) - \alpha p_2(n) \\ &= (1-3\alpha)p_2(n) + (3\alpha-1)p_3(n) \\ &= (1-3\alpha)\,[p_2(n) - p_3(n)] \end{aligned}$$

But from Equation 5.2

$$\delta(n+1) = (1-3\alpha)\delta(n)$$

Now it can easily be shown by induction that[4]

$$\delta(n) = (1 - 3a)^n \delta(0)$$

But from our assumption about the initial distribution (Equations 4.7)

$$\delta(0) = 1$$

hence,

$$\delta(n) = (1 - 3a)^n \tag{5.3}$$

Now

$$p(n + 1) = p_1(n + 1) + p_2(n + 1)$$

If we substitute for the right side of this equation, the expressions in Equations 5.1 yield

$$p(n + 1) = p_1(n) + a p_2(n) + (1 - 2a)p_2(n) + a p_3(n)$$

This can be rewritten as

$$p(n + 1) = p_1(n) + p_2(n) - a[p_2(n) - p_3(n)]$$

From Equations 4.4 and 5.2,

$$p(n + 1) = p(n) - a\,\delta(n)$$

But substituting the value of $\delta(n)$ from Equation 5.3, we obtain

$$p(n + 1) = p(n) - a(1 - 3a)^n \tag{5.4}$$

Now let us write out $p(n)$ for the first few values of n, again remembering that $p(n) = p_1(n) + p_2(n)$. To write the initial term of this sequence, we make use of the fact that $p_2(0) = 1$ (Equations 4.7) and then use the transition equations (5.1):

$$p(0) = 1$$

$$p(1) = a + (1 - 2a) = 1 - a$$

$$p(2) = a + a(1 - 2a) + a^2 + (1 - 2a)^2$$

$$= 1 - 2a + 3a^2$$

$$= 1 - a - a(1 - 3a)$$

$$\begin{aligned} p(3) &= a + a(1 - 2a) + a(1 - 2a)^2 + a^3 \\ &\quad + a^2(1 - 2a) + 2a^2(1 - 2a) + (1 - 2a)^3 \\ &= 1 - 3a + 9a^2 - 9a^3 \\ &= 1 - a + (-2a + 9a^2 - 9a^3) \\ &= 1 - a - a(1 - 3a) - a(1 - 6a + 9a^2) \\ &= 1 - a - a(1 - 3a) - a(1 - 3a)^2 \end{aligned}$$

This soon becomes quite laborious. Thus far, however, we have illustrated Equation 5.4 for the first three terms of the sequence. More important is the fact that writing out these few terms suggests a general expression for p(n):

$$p(n) = 1 - a - a(1 - 3a) - \cdot \cdot \cdot - a(1 - 3a)^{n-1} \qquad (5.5)$$

Equation 5.5 is the sum of a finite series. The fact that it is the correct expression for p(n) can easily be verified by induction. Now if we let $Z = (1 - 3a)$, Equation 5.5 becomes

$$p(n) = 1 - a\sum_{i=0}^{n-1} Z^i, \quad n = 1, 2, \cdot \cdot \cdot, N$$

where N is the number of trials. We now make use of an equality that is quite important here and for the remainder of this development, namely:

$$\sum_{i=0}^{n-1} Z^i = \frac{1 - Z^n}{1 - Z} \qquad (5.6)$$

Hence,

$$\begin{aligned} p(n) &= 1 - a\,\frac{1 - Z^n}{1 - Z} \\ &= 1 - a\left[\frac{1 - (1 - 3a)^n}{1 - (1 - 3a)}\right] \\ &= 2/3 + 1/3(1 - 3a)^n \qquad (5.7) \end{aligned}$$

This completes the demonstration.

Having found this expression for p(n), we proceed to sum over all values of n to obtain $E(\sum X_n)$. It is necessary to demonstrate that $\sum p(n) = 2/3\,N + 1/9a - 1/3$.

$$\sum_{n=1}^{N} p(n) = \sum_{n=1}^{N} [2/3 + 1/3(1 - 3a)^n] = 2/3\,N + 1/3 \sum_{n=1}^{N} (1 - 3a)^n \tag{5.8}$$

The summation is from n = 1 to N to correspond to the observed sequence of critical trials. Let $Z = 1 - 3a$; then,

$$\sum_{n=1}^{N} p(n) = 2/3\,N + 1/3 \sum_{n=1}^{N} Z^n$$

where $\sum_{n=0}^{N} Z^n$ is a geometric series whose sum is $\dfrac{Z^{N+1} - 1}{Z - 1}$; but when n = 0, Z = 1. Therefore, by subtracting 1 from this sum, we obtain an expression for the desired sum:

$$\sum_{n=1}^{N} Z^n = \frac{Z^{N+1} - 1}{Z - 1} - 1 \tag{5.9}$$

Using Equation 5.9 and substituting for Z, we obtain after some simplification

$$\sum_{n=1}^{N} p(n) = 2/3\,N - 1/3 + 1/3 \left[\frac{(1 - 3a)^{N+1}}{-3a} + 1/3a\right]$$

As N becomes large, $(1 - 3a)^{N+1}$ approaches zero. Therefore, for large N,

$$\sum p(n) = 2/3\,N + 1/9a - 1/3 \tag{5.10}$$

and our proof is complete.

We have noted that $\sum p(n) = \sum E(X_n)$; hence we estimate a by equating Equation 5.10 to the observed mean number of correct responses. It will be remembered from Table 2.1 that this observed value was 25.24 for the Moderate condition, whereas the number of trials was 36. Substituting these values in Equation 5.10, we obtain

$$25.24 = 24 + 1/9a - 1/3$$

$$\hat{a} = .07$$

where the superscript ^ indicates that the quantity involves an estimate from data.

Before deriving additional statistics from the model, it is possible to use this estimate of α to make some initial tests of the model.[5] At this point we are interested in two properties of the model which will be compared with data from the Moderate condition. These are (1) the trial-by-trial probability of a correct response, and (2) the frequency distribution of errors.

The theoretical mean curve is obtained by inserting the estimated value of α in Equation 5.7 and evaluating the expression for all values of n. The theoretical curve thus arrived at is superimposed on the observed curve (Figure 2.1) in Figure 5.1.

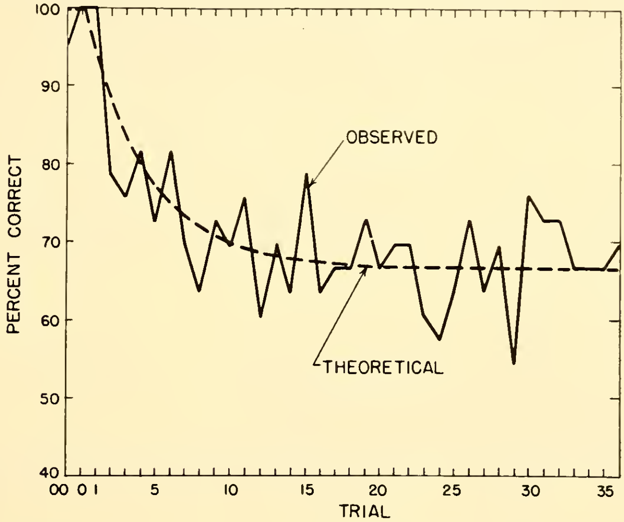


Figure 5.1. Comparison of the theoretical and observed mean curves. (One-parameter model: $\alpha = .07$).

The next property to consider is the distribution of errors yielded by the model. Although we have fit the mean of this distribution in estimating α, this method of estimation has not imposed any restriction on the shape of the error distribution. As Chapter 3 demonstrated, a model that is fitted to the mean curve need not predict the shape or the variance of the distribution of errors. In the present case, we did not obtain the best fit to the mean curve, but rather fit only the mean of the distribution of errors,[6] so that there is less reason to expect the model to have a distribution similar to that observed in the experiment.

At this point computational difficulties arise. We have already indicated the problem involved in deriving an expression for the variance of the distribution of errors (the variance of a sum, cf.

Chapter 3) when we are dealing with sequential dependency. The complexity involved in deriving the distribution function for the distribution of errors is as forbidding. In principle, it is possible to derive the asymptotic distribution function, but as in many other cases it is easier to assert the principle than to accomplish the derivation. To illustrate the procedure and the difficulty involved, we shall derive expressions for a few points of this function.

Let Π_k = asymptotic probability of precisely k errors,[7] $k = 0, 1, 2, \cdots, 36$.

We shall illustrate the derivations for k = 0, 1, and 2. For $k = 0$:

According to the model a subject can make zero errors by remaining in state S_2 for a number of trials and them moving to S_1. That is, by a sequence of states such as:

2 2 ------------------ 2 1

where there are i + 1 trials in S_2, and $i = 0, 1, 2 \cdots$. Since the transition probabilities from S_2 to S_1 and S_2 to S_2 are a and $1 - 2a$, respectively, the probability of such a sequence is given by

$$(1 - 2a)^i a$$

(Note that the exponent is i rather than i + 1 because all subjects are assumed to be in S_2 on trial 0.)

We are interested in the probability of all such sequences; because these sequences are mutually exclusive, the desired quantity is

$$\Pi_0 = \sum_{i=0}^{\infty} (1 - 2a)^i a \qquad (5.11)$$

To evaluate this expression, we use the following equality:

$$\sum_{i=0}^{\infty} Z^i = \frac{1}{1 - Z} \qquad (5.12)$$

Because a is a constant, we can write

$$\Pi_0 = a \sum_{i=0}^{\infty} (1 - 2a)^i$$

and using Equation 5.12, we obtain

$$\Pi_0 = a\left[\frac{1}{1-(1-2a)}\right] = \frac{a}{2a} = \frac{1}{2} \tag{5.13}$$

For k = 1:

We need to sum over all sequences of the type

$$\underbrace{2\text{----------}2}_{i+1}\ \ 3\ \ \underbrace{2\text{----------}2}_{j+1}\ \ 1\text{-----}$$

where there are i + 1 trials in S_2 before the single trial in S_3 and j + 1 trials after it. That is, the only way a subject can make precisely one error is by remaining in S_2 for some number of trials, moving to S_3 for a single trial, moving back to S_2 for a number of trials, and finally moving to S_1. The probability of any sequence of this type is then

$$\Pi_1 = \left[\sum_{i=0}^{\infty}(1-2a)^i\right]a \cdot a\left[\sum_{j=0}^{\infty}(1-2a)^j\right]a$$

$$= a^3\sum_{i=0}^{\infty}(1-2a)^i\sum_{j=0}^{\infty}(1-2a)^j \tag{5.14}$$

Applying Equation 5.12 to each summation, we get

$$\Pi_1 = \frac{a^3}{[1-(1-2a)][1-(1-2a)]} = \frac{a^3}{4a^2} = \frac{a}{4} \tag{5.15}$$

Since we estimate $a = .07$,

$$\hat{\Pi}_1 = .0175 \tag{5.16}$$

For k = 2:

At this point the complexities begin to arise. Two different types of sequences can lead to exactly two errors. Hence, the probability of precisely two errors is the probability of either a sequence of type I or a sequence of type II, which is the sum of the probabilities of respective types because these are mutually exclusive. This it is necessary to sum over

$$\underbrace{2\text{----------}2}_{i+1}\ \ 3\ \ \underbrace{2\text{----------}2}_{j+1}\ \ 3\ \ \underbrace{2\text{----------}2}_{k+1}\ \ 1\text{-----}$$

and

$$\underbrace{2\text{----------}2}_{r+1}\ 3\ \ 3\ \underbrace{2\text{----------}2}_{s+1}\ 1\text{----------}$$

Hence,

$$\Pi_2 = \left[\sum_{i=0}^{\infty}(1 - 2\alpha)^i\right] \alpha^2 \left[\sum_{j=0}^{\infty}(1 - 2\alpha)^j\right] \alpha^2 \left[\sum_{k=0}^{\infty}(1 - 2\alpha)^k\right] \alpha$$

$$+ \left[\sum_{r=0}^{\infty}(1 - 2\alpha)^r\right] \alpha^2 (1 - 2\alpha) \left[\sum_{s=0}^{\infty}(1 - 2\alpha)^s\right] \alpha$$

$$= \frac{1}{(2\alpha)^3}(\alpha^5) + \frac{1}{(2\alpha)^2}\alpha^3(1 - 2\alpha)$$

$$= \frac{\alpha(2 - 3\alpha)}{8}$$

Substituting .07 for α gives

$$\hat{\Pi}_2 = .0157 \tag{5.17}$$

We have thus illustrated the increasing complexity of deriving expressions for points of the distribution function. The only result of which we shall make use below is $\Pi_0 = \frac{1}{2}$, Equation 5.13. The reader should note that this value, the probability of preciesely zero errors, is independent of α; that is, the one-parameter model always leads to this probability, regardless of the estimate of α.

As k increases, the number of sequences leading to precisely k errors increases up to a maximum when k = 18. The labor involved in computing this theoretical distribution function is easily seen. Therefore, it is more efficient to test the model by using Monte Carlo computations. That is, we run a sample of Monte Carlo subjects, compute the desired statistics, and compare these with the same statistics computed from the sample of real subjects.

The Monte Carlo data are generated in the following way:

1. On trial 0, the "stat-subject" is in state S_2. A table of random numbers (Rand, 1955) is entered to determine the state the subject moves to on trial 1. Three-digit random numbers are used.

2. If the subject is in S_2, the transition rules applied are
   a. If $930^8 \leq$ Random Number $\leq 999,^8$ the subject moves to S_1.
   b. If $070 \leq$ Random Number ≤ 929, the subject stays in S_2.
   c. If $000 \leq$ Random Number ≤ 069, the subject goes to S_3.

This determines the subject's position on trial 1.

3. If the first subject is in S_1 on trial 1, we start the next subject. If this first subject is in S_2 or S_3,[9] we select the next random number. If he is in S_2, the preceding rules are reapplied, but if he is in S_3 the rules under 4 are applied:

4. If the subject is in S_3 the transition rules are
   a. If $930 \leq$ Random Number ≤ 999, the subject moves to S_2.
   b. If $070 \leq$ Random Number ≤ 929, the subject stays in S_3.
   c. If $000 \leq$ Random Number ≤ 069, the subject moves to S_4.

To decide a subject's state on trial $n + 1$, the preceding rules are applied to the random number drawn from the table. Depending on whether the subject was in S_2 or S_3 on trial n, paragraph 2 or paragraph 4 is applied. These decisions are made repeatedly until the subject moves to S_1 or S_4 and is absorbed or until he has completed 36 critical trials. When one "stat-subject" has been absorbed or has completed 36 trials, the decisions begin for the next "stat-subject." In these Monte Carlo computations, we use consecutive random numbers until we have a sample of 33 subjects. For each new sample, the table of random numbers is entered at a different place chosen at random.

The procedure just outlined yields a sequence of states for each subject. To compare these data with the data from actual subjects, it is necessary to convert these sequences of states to sequences of responses. By using Equation 4.2, it is easy to accomplish this conversion.

To compare the model's predictions with the data, 10 Monte Carlo samples of 33 subjects each were run. A large number of "stat-subjects" were run in order to get stable estimates of the population values of the model's predictions. In addition, the Monte Carlo subjects were run in 10 separate samples rather than in a single group of 330 subjects to enable us to get some information about the sampling distributions of the statistics involved.

Table 5.1 shows the comparison of the frequency and percentage distributions of Monte Carlo and actual subjects. The data are grouped in class intervals for convenience of presentation.

Table 5.1. Comparison of the Distribution of Errors from the Monte Carlo Subjects with That Observed in the Moderate Condition

(One-Parameter Model: $\alpha = .07$)

Number of Errors	Monte Carlo Subjects		Actual Subjects	
	Mean Frequency	Mean Per Cent	Frequency	Per Cent
0-4	19.0	57.6	16.0	48.5
5-9	1.7	5.1	2.0	6.1
10-14	.7	2.1	3.0	9.1
15-19	1.4	4.2	4.0	12.1
20-24	1.8	5.4	2.0	6.1
25-29	2.9	8.7	1.0	3.0
30 +	5.5	16.7	5.0	15.2
$\bar{X}$	10.7		10.8	
s^2	184.81		143.38	

The theoretical values are based on a sample size of 33 and are obtained by dividing the total frequency in the 10-sample run by 10. In contrast to those models discussed in Chapter 3, this model predicts a distribution of errors that has the proper shape and a variance of the correct order of magnitude. It should be noted that the variance predicted by the model is larger than observed in the experiment. In addition, the model overestimates both tails of the distribution and consequently underestimates the middle.

For an indication of how much of the discrepancy between observed and predicted is due to sampling variation, we can examine the sample values of the mean and variance for each of the 10 Monte Carlo samples. Another statistic whose sampling variation is of interest is $\Pi_0 M$ (M = sample size), the number of subjects making precisely zero errors in 36 trials. This statistic is chosen because of the significance attached to "complete independence" (Asch, 1956). Examining sample results for these statistics may indicate an area in which there is an important discrepancy between the model and the experimental data.

The sample values of $\bar{X}$, s^2, and $\Pi_0 M$ appear in Table 5.2. Both the observed mean and the observed variance fall within the range of values in Table 5.2. It is not surprising that we can regard the observed mean as drawn from the distribution in the table, for this statistic was used to estimate α. The fact that the variance also falls in the range of tabled sample values, however, represents a successful prediction of the model. On the other hand, the observed value of $\Pi_0 M$ lies outside the range

Table 5.2. Sample Values of $\overline{X}$, s^2, and $\Pi_0 M$ from 10 Monte Carlo Samples

(Moderate Condition; One-Parameter Model: $\alpha = .07$)

Sample	$\overline{X}$	s^2	$\Pi_0 M$
1	8.3	158.7	20
2	14.3	216.8	14
3	6.9	118.6	16
4	10.5	187.0	19
5	9.9	191.1	19
6	13.8	207.6	13
7	13.1	218.0	15
8	13.6	209.1	14
9	9.2	179.4	18
10	7.2	139.0	20
(Actual Subjects)	(10.8)	(143.38)	(10)

of values computed from the Monte Carlo samples. Since we computed $\Pi_0 = \frac{1}{2}$, Equation 5.13, the expected value of $\Pi_0 M$ for a sample of size 33 is 16.5, which agrees closely with the mean of the sample values in Table 5.2 (16.8). Furthermore, when $\Pi_0 = \frac{1}{2}$, $\Pi_0 M$ should have its largest variance; since the variance of the distribution in Table 5.2 is 3.61 and the standard deviation 1.9, the probability is small that the observed value of $\Pi_0 M$ was drawn from this distribution. We conclude, then, that the one-parameter model overestimates the value of this statistic. Modifying the model to improve the prediction of $\Pi_0 M$ may also improve the fit of the entire theoretical distribution function to the observed distribution of errors.

Before we discuss modification of the model, it is desirable to compute the expected values of a few additional statistics and compare these with the experimental data. The remaining properties we should like to examine relate to the asymptotic probability of a correct response and to the distribution of alternations. We shall compute the asymptotic probability of a correct response and the limiting distribution of subjects among the four states. With regard to alternations, we shall derive the distribution function for the distribution of alternations as well as the mean and variance of this distribution. Following each derivation, we shall present the observed values of the statistics derived and comment on the fit of observed to theoretical.

Equation 5.7, the expression for p(n), is the basic tool for determining the asymptotic probability of a correct response. This asymptotic probability is denoted by $p(\infty)$ and

$$p(\infty) = \lim p(n), \quad n \to \infty \tag{5.18}$$

Substituting Equation 5.7 in this equation, we have

$$p(\infty) = \lim \left[\tfrac{2}{3} + \tfrac{1}{3}(1 - 3a)^n \right], \quad n \rightarrow \infty$$

but as n gets large,

$$(1 - 3a)^n \rightarrow 0$$

hence,

$$p(\infty) = \tfrac{2}{3} \tag{5.19}$$

The form of the Equation 5.7 is such that p(n) is a monotonic decreasing function of n. We now know that this function approaches an asymptote of $\frac{2}{3}$. The theoretical curve of p(n), presented in Figure 5.1, illustrates these properties and also shows that the theoretical rate of change of p(n) is monotone decreasing, approaching zero as an asymptote. It would be desirable to determine whether or not the curve of the observed proportion of correct responses also has these properties. However, it is not practicable to attempt this test because the sampling variability of each point on the observed curve is large relative to the theoretical rate of change. But when p(n) approaches an asymptote, there are additional testable consequences.

For this model, p(n) reaching an asymptote implies that the process of change of response has ended, that all subjects have been absorbed in either state S_1 or state S_4. This can easily be shown. The following are expressions for $p_j(n)$, $j = 1, 2, 3, 4$; that is, for the probability of being in state S_j on trial n. (The derivation of these expressions is not shown because the algebra is quite lengthy.) They are derived, however, from Equations 5.7 and 5.1:

$$\left.\begin{aligned} p_1(n) &= \tfrac{2}{3} - \tfrac{1}{2}(1 - a)^n - \tfrac{1}{6}(1 - 3a)^n \\ p_2(n) &= \tfrac{1}{2}(1 - a)^n + \tfrac{1}{2}(1 - 3a)^n \\ p_3(n) &= \tfrac{1}{2}(1 - a)^n - \tfrac{1}{2}(1 - 3a)^n \\ p_4(n) &= \tfrac{1}{3} - \tfrac{1}{2}(1 - a)^n + \tfrac{1}{6}(1 - 3a)^n \end{aligned}\right\} \tag{5.20}$$

To determine the asymptotic value of these expressions, that is, $p_j(\infty)$, we take the limit as $n \rightarrow \infty$ of each of the equations in Equations 5.20. The result is

$$\left.\begin{aligned} p_1(\infty) &= \tfrac{2}{3} \\ p_2(\infty) &= 0 \\ p_3(\infty) &= 0 \\ p_4(\infty) &= \tfrac{1}{3} \end{aligned}\right\} \qquad (5.21)$$

Thus, when the asymptote is reached, two-thirds of the subjects should be in state S_1 and one-third in state S_4. But the definition of S_1 and S_4 given earlier implies that a subject in one of these states will give the same response on every trial after the point he entered the state. Let us call such a sequence of like responses a "consistent" sequence. Then as $n \to \infty$, two-thirds of the subjects should have consistent sequences of correct responses, and one-third should have consistent sequences of incorrect responses.

Because it is not accurate to compare a theoretical experiment having an infinite number of trials with an actual experiment of 36 critical trials, we choose, for practical purposes, a few values of n such that the asymptote of p(n) is closely approached. Using Equations 5.20, we can compute the theoretical proportion of subjects absorbed for the chosen values of n and then compare these expected proportions with the proportions absorbed estimated from the actual data.

Estimating from the data the proportion of subjects absorbed at any point in time presents certain difficulties. A person who is absorbed has a consistent sequence of responses from trial n onward, but not all subjects who have consistent sequences beginning on trial n have been absorbed. That is, a subject can have a consistent sequence by remaining in a temporary state for n + t trials and then moving to the absorbing state (characterized by the same response as the temporary state) on trial n + t + 1. Thus, when we observe a consistent sequence of responses, the sequence of states could be

2---------------21----------

or

3---------------34----------

Therefore, if we use the proportion of subjects who have consistent sequences from trial n on, to estimate the proportion absorbed, we must recognize that this estimate provides an upper bound for the proportion absorbed. Because the number of subjects who are consistent following trial n include some who have not been absorbed, an estimate based on these consistent subjects is an estimate of the maximum proportion absorbed at trial n. Furthermore, it is necessary to use a number of trials after trial n in order to decide whether or not a subject is consistent. Thus, it is not possible to estimate the proportion absorbed as of the last experimental trial.

Accordingly, we have chosen to compare the predicted and observed proportions absorbed at trials 10, 20, and 30. Table 5.3 presents these data.

Table 5.3. Predicted and Observed Proportion of Subjects Absorbed on Trial n

(Moderate Condition; One-Parameter Model: $\alpha = .07$)

From Trial Number	Actual Subjects Showing Consistent Sequences		Predicted Percentage Absorbed
	Number	Per Cent	
10	13	39.4	51.6
20	21	63.6	76.6
30	25	75.8	88.7

The important finding in this table is that at each chosen point in time, the model's prediction is about 13 per cent greater than the estimate from the data of the maximum proportion absorbed. We can conclude from this table that the model describes a process of absorption that occurs more rapidly than that seen in the data from the present experiment. Although we are not in a position to estimate the asymptotic distribution of the subjects among the four states and thus test that prediction of the model, it is interesting to note that 18 of the 25 subjects (72 per cent) who have consistent sequences from trial 30 on are consistently correct. If half of the 8 subjects who are inconsistent at this point become consistently correct while the remaining become consistently incorrect, the observed asymptotic distribution will be exactly that of the predicted. Unfortunately, we are not able to tell how likely that event would be as the experiment terminated at trial 36. The important suggestion of this analysis, is, however, that the process has not terminated; hence, to obtain better estimates of the asymptotic properties of the data, it will be necessary to conduct a new experiment with a longer sequence of critical trials.

The final properties of the one-parameter model to be considered concern the distribution of alternations. An alternation, it will be remembered, occurs when a subject gives response

R_i on one trial and then R_j $(j \neq i)$ on the trial immediately following. For example, if he responds correctly on trial n, and incorrectly on n + 1, we call this an alternation. Here we are concerned with the distribution function, the mean and the variance of the number of alternations. We first derive the theoretical values of these statistics and then test them against the data from the Moderate condition.

Let A be a random variable, and let $\phi_k = \Pr\{A = k\}$, that is, the probability of precisely k alternations. To derive an expression for ϕ_k, we proceed in a manner similar to that in deriving expressions for Π_k (the probability of exactly k errors) given earlier. But in the present case, it is relatively easy to obtain a general expression for ϕ_k, whereas it was too laborious to attempt for Π_k. First let us write the sequence of states which lead to k alternations for a few values of k:

k	Sequence
0	2-----21-----
1	2-----23-----34-----
2	2-----23-----32-----21-----
3	2-----23-----32-----23-----34-----
4	2-----23-----32-----23-----32-----21-----

It will be remembered from Equation 5.12 that the probability of any sequence of the form

2-----2

or

3-----3

where there are i + 1 2's or 3's, is given by

$$\sum_{i=0}^{\infty} (1 - 2a)^i = \frac{1}{2a}$$

To obtain the probability of a sequence that leads to zero alternations, we multiply the probability of a subsequence of S_2's, the probability of moving from S_2 to S_1, and the probability of a subsequence of S_1's together. Since this last is

$$\Pr\{S_1 \mid S_1\} \cdot \cdot \cdot \Pr\{S_1 \mid S_1\}$$

and $\Pr\{S_1 \mid S_1\} = 1.00$, we have

$$\Pr\{A = 0\} = \frac{1}{2a}\, a = \tfrac{1}{2} \tag{5.22}$$

The probabilities of the sequences leading to 1, 2, 3, or 4 alternations are computed in a similar fashion. Each subsequence has a known probability; the probability of the complete sequence is simply the product of the probabilities of the component subsequences. Thus, using the sequence given for $k = 4$, we find $\Pr\{A = 4\} = a^5/(2a)^5 = 1/32$ because there are five transitions and five subsequences of the form 2-----2 or 3-----3. It should also be noted that there is only one form of sequence that leads to exactly a given number of alternations. (This is the important difference between the problem in deriving Π_k and that involved in deriving ϕ_k.)

Computing the probabilities of the sequences of our illustration, we have

$$\Pr\{A = 0\} = \tfrac{1}{2}$$

$$\Pr\{A = 1\} = \frac{a^2}{(2a)^2} = \tfrac{1}{4}$$

$$\Pr\{A = 2\} = \frac{a^3}{(2a)^3} = \tfrac{1}{8}$$

$$\Pr\{A = 3\} = \frac{a^4}{(2a)^4} = \tfrac{1}{16}$$

$$\Pr\{A = 4\} = \tfrac{1}{32}$$

This suggests that the general form of ϕ_k is

$$\phi_k = \frac{1}{2^{k+1}}, \quad k = 0,\ 1,\ 2, \cdots \tag{5.23}$$

That this is the correct expression can be proven by induction.

We have obtained an expression for the distribution function that is independent of the parameter a.

We shall now demonstrate that the mean of the distribution of alternations is equal to 1.00 and the variance is equal to 2.00.

$$E(A) = \sum_{k=0}^{\infty} k\phi_k$$

$$= \sum_{k=0}^{\infty} \frac{k}{2^{k+1}} = \tfrac{1}{2} \sum_{k=0}^{\infty} \frac{k}{2^k} \tag{5.24}$$

If we let $Z = \frac{1}{2}$, we have

$$E(A) = \frac{1}{2} \sum_{k=0}^{\infty} kZ^k$$

and $\sum_{k=0}^{\infty} kZ^k$ is a well-known series whose sum is

$$\sum_{k=0}^{\infty} kZ^k = \frac{Z}{(1 - Z)^2} \qquad (5.25)$$

hence,

$$E(A) = \frac{1}{2}\left[\frac{\frac{1}{2}}{(\frac{1}{2})^2}\right] = 1.00 \qquad (5.26)$$

Similarly,

$$\sigma^2(A) = E(A^2) - [E(A)]^2 \qquad (5.27)$$

and

$$E(A^2) = \sum_{k=0}^{\infty} k^2 \phi_k = \sum_{k=0}^{\infty} \frac{k^2}{2^{k+1}}$$

$$= \frac{1}{2} \sum_{k=0}^{\infty} \frac{k^2}{2^k} \qquad (5.28)$$

Again letting $Z = \frac{1}{2}$, we have

$$E(A^2) = \frac{1}{2} \sum_{k=0}^{\infty} k^2 Z^k$$

but $\sum_{k=0}^{\infty} k^2 Z^k$ is also known:

$$\sum_{k=0}^{\infty} k^2 Z^k = \frac{2Z^2}{(1 - Z)^3} + \frac{Z}{(1 - Z)^2} \qquad (5.29)$$

therefore,

$$\frac{1}{2}\sum_{k=0}^{\infty}\frac{k^2}{2^k} = \frac{1}{2}\left[\frac{2\left(\frac{1}{4}\right)}{\left(\frac{1}{2}\right)^3} + \frac{\frac{1}{2}}{\left(\frac{1}{2}\right)^2}\right] = 3.00 \qquad (5.30)$$

Substituting in Equation 5.27 the values computed for $E(A)$ and $E(A^2)$, that is, Equations 5.26 and 5.30, we find

$$\sigma^2(A) = 3 - 1^2 = 2.00 \qquad (5.31)$$

We have computed the mean and variance of the distribution of alternations for the one-parameter model and have shown that these two properties are independent of α. The theoretical values are $u_a = 1.00$ and $\sigma_a{}^2 = 2.00$.

These derivations enable us to test further the one-parameter model. Table 5.4 presents the theoretical and observed frequency

Table 5.4. Expected and Observed Distribution of Alternations

(One-Parameter Model)

Number of Alternations (k)	Probability (ϕ_k)	Expected Frequencies ($33\phi_k$)	Observed Frequencies
0	.5	16.50	10
1	.25	8.25	1
2	.125	4.13	3
3	.062	2.07	0
4	.031	1.04	2
5	.016	0.52	4
6	.008	0.26	2
7	.004	0.13	1
8	.002	0.07	0
9	.001	0.03	0
10	.000	0.00	0
11	.	.	0
12	.	.	0
13	.	.	1
14	.	.	1
15	.	.	0
16	.	.	3
17	.	.	2
18	.	.	1
19	.	.	0
20	.	.	1
21	.	.	0
22	.	.	0
23	0.000	0.00	1
$E(A) = 1.00$		$\overline{X} = 6.79$	
$\sigma^2(A) = 2.00$		$s^2 = 53.42$	

distributions of alternations, and the means and variances of these distributions. We arrive at the expected frequencies in this table by multiplying the probability of k alternations by 33, the size of the sample of actual subjects.

It is clear from the table that the one-parameter model inadequately describes the observed data. In the first place, the model underestimates the observed mean by a factor of 6. Second, the model predicts a concentration about the mean, that is, a small variance, whereas the observed values are not clustered about their mean. Examining the distributions in detail indicates further discrepancies. The theoretical distribution has a higher mode, drops off earlier, and hence has a lower tail than the observed. The higher mode follows from the fact that the model overestimates the number of subjects who make no errors (hence no alternations). The other discrepancies, however, are not easily explained. The higher tail of the observed distribution suggests that with respect to alternations the sample of observed subjects may not be homogeneous. We shall return to this point in later analyses.

The important result of this analysis is that the model predicts considerably more consistency on the part of subjects than we observe in the data. For example, if a subject responds to one of the pressures acting on him at some point in time, he should continue to do so for a larger number of trials according to the model. This is clearly not the case for a substantial proportion of subjects.

This completes the examination of the one-parameter model and the testing of this model with data based on the Moderate condition. Before discussing the results of this examination, we should comment on why we have not tested this special case of the "conflict model" with data from the Extreme condition. Detailed consideration of the data from the Extreme condition is not required; it is readily apparent that this model does not fit the Extreme condition. In the first place, as we noted earlier, the one-parameter model predicts that p(n) is a monotonic decreasing function of n. Second, this model predicts, independent of the value of the parameter α, an asymptotic probability of a correct response equal to two-thirds. It is clear from the trial-by trial proportion of correct responses in the Extreme condition (Figure 2.1) that the observed proportion is not a monotonic function of n; p(n) decreases at first, then shows an increasing trend. Furthermore, this proportion is not approaching an asymptote of two-thirds. Thus it is unnecessary to be concerned with other properties of the data in order to conclude that the one-parameter model is grossly inadequate as a description of the Extreme condition.

In an effort to describe the process of change in the probability of a correct response, we have examined a model which describes a process of absorption. In addition to estimating α, we have considered the following properties of the model:

1. The mean curve of the probability of a correct response.
2. The distribution of errors, particularly the probability of making precisely zero errors.
3. The variance of the distribution of errors.
4. The asymptotic probability of a correct response and the rate of absorption.
5. The distribution of alternations and its mean and variance.

Although no exact tests of the goodness of fit of this model to the data from the Moderate condition were presented, several conclusions are fairly clear. The theoretical mean curve is a good approximation to that observed; the shape and variance of the distribution of errors are closer to the observed than were the predictions of any of the baseline models discussed in Chapter 3. In fact, study of the Monte Carlo sampling distribution of the variance of the distribution of errors indicated that the observed value of this statistic could have come from this sampling distribution. On the other hand, the model overestimated both tails of the observed error distribution, predicted a process of absorption occurring more rapidly than was seen in the data (that is, more of the subjects should have been absorbed at each time than actually were), and led to a distribution of alternations that differed considerably from the observed.

While the one-parameter model inadequately describes the process observed in the Moderate condition, it does describe a related process. This related process is one which terminates more rapidly and in which the subjects show more consistent behavior during any specified interval of time. Since the restriction that all the transitions between the states are equally probable leads to early absorption and a high frequency of consistent sequences, modifying this restriction should improve the fit of the model to the data, providing such modification does not alter other properties of the model.

Analyzing the one-parameter model has posed some important problems concerning the nature of the process under investigation. The model incorporates the premise that all subjects will eventually become consistently correct or consistently incorrect in their responses. It is conceivable, however, that there is an additional equilibrium state, that of "irresolution." Although the majority of subjects do become consistent, those who did not could have remained inconsistent no matter how long the experiment continued. What the analysis does suggest is that the number of trials in the experiment was not sufficient to draw any conclusions about this asymptotic distribution of subjects in the four states. This problem will receive further consideration following the discussion of the remaining models. Finally, in testing the fit of the model, we introduced the number of alternations as an interesting statistic to examine. The question of the relation of this statistic to the conflict interpretation of the model should be raised. This statistic may be an appropriate index of the intensity of conflict; that is, the more intense the conflict is for the subject, the more likely is he to alternate between the possible responses before reaching a resolution. If this supposition is true, then the one-parameter model predicts a conflict of low intensity (i. e., predicts a mean of one alternation) that does not agree with the experimenter's impressions of the situation noted in Chapter 2. We shall see how the other special cases of the model differ in their predictions of this index.

5.3 The Two-Parameter Model

In the discussion of the general model, Chapter 4, the four parameters were α, β, γ, and ϵ. For the one-parameter case, we equated these four; the two-parameter case allows some choice. We can assume one of three sets of equalities:

$$\left.\begin{array}{l} 1.\ \ \alpha = \beta,\ \gamma = \epsilon \\ 2.\ \ \alpha = \gamma,\ \beta = \epsilon \\ 3.\ \ \alpha = \epsilon,\ \beta = \gamma \end{array}\right\} \qquad (5.32)$$

We have chosen the last assumption for two reasons. First let us recall the transitions to which these parameters refer. Writing these as conditional probabilities, we have

$$\begin{aligned} &\Pr\{S_1 \mid S_2\} = \alpha \\ &\Pr\{S_3 \mid S_2\} = \beta \\ &\Pr\{S_2 \mid S_3\} = \gamma \\ &\Pr\{S_4 \mid S_3\} = \epsilon \end{aligned}$$

The previous section suggested a modification of the one-parameter model which would allow a slower rate of absorption and also more alternations. Since all subjects are in state S_2 initially, we require transition probabilities such that a greater proportion of subjects moved to S_3 than were absorbed in S_1 on the first critical trial and on subsequent trials. To prolong the process, it is desirable that a subject who is about to move would be more likely to move to another temporary state rather than to an absorbing state. Hence, we require

$$\beta > \alpha \text{ and } \gamma > \epsilon \qquad (5.33)$$

And it is clear that the only one of the three sets of equalities in Equations 5.32 which meets this requirement is the third one.[10] In addition, letting $\alpha = \epsilon$ and $\beta = \gamma$ preserves a symmetry that enables us to derive certain expressions not possible otherwise.

The basic tool in this development is the set of transition equations. Analogous to Equations 5.1 we have

$$\left.\begin{aligned} p_1(n+1) &= p_1(n) + \alpha p_2(n) \\ p_2(n+1) &= (1-\alpha-\beta)p_2(n) + \beta p_3(n) \\ p_3(n+1) &= (1-\alpha-\beta)p_3(n) + \beta p_2(n) \\ p_4(n+1) &= p_4(n) + \alpha p_3(n) \end{aligned}\right\} \qquad (5.34)$$

The difference between this set and Equations 5.1 lies in the second and third equations. Instead of $(1 - 2\alpha)$ as a coefficient of the first term of each expression, we now have $(1 - \alpha - \beta)$, and instead of α as the coefficient of the second term, we now have β. Figure 5.2, diagram of the states and transitions for this model, clarifies these differences.

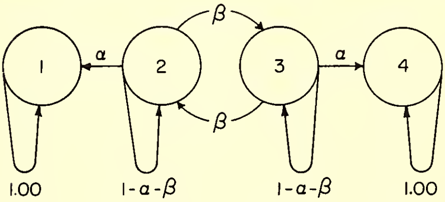


Figure 5.2. States and transitions for the two-parameter model.

In order to estimate the parameters, we require two independent expressions that we can equate to statistics computed from the data. That is, we must find two simultaneous equations to solve for the two unknowns, α and β. As in the one-parameter case, it is possible to derive an expression for $E(\sum X_n)$ in terms of the parameters. For the second equation, we shall derive an expression for Π_0.

For the two-parameter model, the derivation of $E(\sum X_n)$ is accomplished in the same way as it was in the one-parameter case. Since $E(X_n) = p(n)$, and $E(\sum X_n) = \sum E(X_n)$, we find an expression for $p(n)$ and sum this for all values of n.

We shall now show that

$$\sum_{n=1}^{N} p(n) = \frac{\beta + \alpha}{2\beta + \alpha} N - \frac{\beta}{2\beta + \alpha} + \frac{\beta}{(2\beta + \alpha)^2}$$

To accomplish this, we must first prove that p(n) is given by

$$\frac{\beta + \alpha}{2\beta + \alpha} + \frac{\beta}{2\beta + \alpha}(1 - 2\beta - \alpha)^n$$

Again let

$$\delta(n) = p_2(n) - p_3(n)$$

Subtracting the third equation from the second equation in Equations 5.34, we have

$$\delta(n + 1) = (1 - 2\beta - \alpha)\delta(n)$$

and it is easily shown that[11]

$$\delta(n) = (1 - 2\beta - \alpha)^n$$

In the same way as we arrived at Equation 5.4, we have

$$p(n + 1) = p(n) - \beta(1 - 2\beta - \alpha)^n \tag{5.35}$$

Letting

$$Z = (1 - 2\beta - \alpha)$$

$$p(n) = 1 - \beta \sum_{i=0}^{n-1} Z^i \qquad n = 1, 2, \cdots, N \tag{5.36}$$

The reader can verify Equation 5.36 by writing out p(n) for the first few values of n. Applying Equation 5.6, substituting for Z, and simplifying lead to

$$p(n) = \frac{\beta + \alpha}{2\beta + \alpha} + \frac{\beta}{2\beta + \alpha}(1 - 2\beta - \alpha)^n \tag{5.37}$$

Next we want to sum p(n) as n goes from 1 to 36. If we let

$$Z = (1 - 2\beta - \alpha)$$

we can again apply Equation 5.9, with the result

$$\sum_{n=1}^{N} p(n) = \frac{\beta + \alpha}{2\beta + \alpha} N - \frac{\beta}{2\beta + \alpha} + \frac{\beta}{(2\beta + \alpha)^2} + \frac{(1 - 2\beta - \alpha)^{N+1}}{-2\beta - \alpha} \tag{5.38}$$

But for large N, the final term is negligible; hence, the first equation for the estimation of α and β is

$$\sum_{n=1}^{N} p(n) = \frac{\beta + \alpha}{2\beta + \alpha} N - \frac{\beta}{2\beta + \alpha} + \frac{\beta}{(2\beta + \alpha)^2} \tag{5.39}$$

This completes the derivation of one of the two equations necessary to estimate the parameters of this model.

An expression for the asymptotic probability of precisely zero errors, Π_0, will give a second independent equation. As was pointed out earlier, a subject can make precisely zero errors by remaining in S_2 for i + 1 trials and then moving to S_1. The probability of a given sequence of this form for the two-parameter model is

$$(1 - \alpha - \beta)^i \alpha$$

and thus the probability of any sequence of this type is the sum over i of this quantity. Hence,

$$\Pi_0 = \alpha \sum_{i=0}^{\infty} (1 - \alpha - \beta)^i \tag{5.40}$$

which when evaluated using Equation 5.12 is

$$\Pi_0 = \frac{\alpha}{\alpha + \beta} \tag{5.41}$$

Using Equations 5.39 and 5.40, we can estimate α and β. For the Moderate condition, the observed mean number of correct responses was 25.24, and the observed proportion of subjects who made zero errors in 36 trials was .30. Hence, we have the following system of simultaneous equations.

$$\left.\begin{aligned} 25.24 &\triangleq 36\,\frac{\beta + \alpha}{2\beta + \alpha} - \frac{\beta}{2\beta + \alpha} + \frac{\beta}{(2\beta + \alpha)^2} \\ .3 &\triangleq \frac{\alpha}{\alpha + \beta} \end{aligned}\right\} \tag{5.42}$$

From the second equation, we get $\beta \triangleq 7/3\alpha$, which substituted in the first yields

$$\hat{\alpha} = .016$$

$$\hat{\beta} = .038$$

The first test of the two-parameter model is made by plotting the trial-by-trial probability of a correct response and comparing this with the mean curve (trial-by-trial proportion of correct responses) from the data. The theoretical curve results from substituting the estimates of α and β in Equation 5.37 and evaluating the expression repeatedly for n = 1 to n = 36. After substitution the expression to be evaluated is

$$p(n) = .59 + .41\,(.91)^n, \quad n = 1, 2, \cdots, 36$$

This expression immediately suggests two properties of the model: (1) p(n) is, as in the one-parameter model, a monotonic function of n; and (2) the asymptotic probability of a correct response, according to these estimates of the parameters, is .59. These properties are illustrated in Figure 5.3, which shows the theoretical p(n) and also the observed proportion of correct responses.

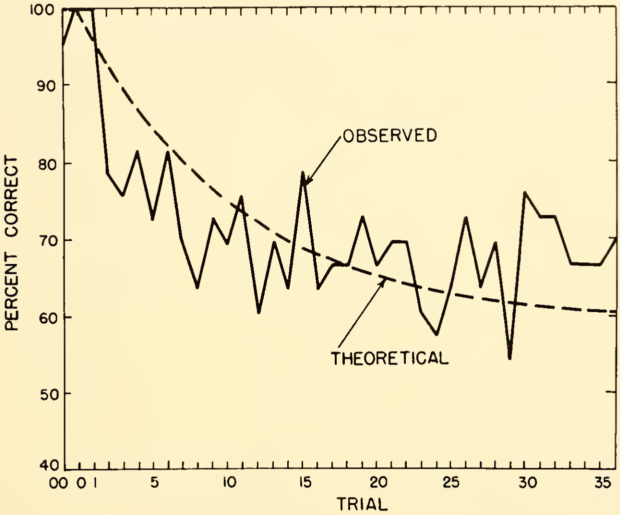


Figure 5.3. Comparison of the theoretical and observed mean curves. (Two-parameter model: $\alpha = .016$, $\beta = .038$)

The fit of theoretical to observed shown in this figure is disappointing. We achieved a better fit with the one-parameter model. In this figure, the theoretical curve descends more slowly than the observed and continues descending when the observed proportion appears to have stabilized. A major difficulty is that the theoretical asymptote is considerably lower than we should estimate the actual asymptote to be. This discrepancy suggests that another method of estimating the parameters would perhaps improve the fit. We could, for example, use the asymptotic probability of a correct response instead of Π_0. Such an estimation might improve the fit of the mean curve without creating too much of a discrepancy between the theoretical and observed values of $\Pi_0 M$ (the number of subjects making 0 errors). From Equation 5.37, taking the limit as n → ∞, we find

$$p(\infty) = \frac{\beta + \alpha}{2\beta + \alpha} \tag{5.43}$$

If we estimate this probability from the data as $\frac{2}{3}$, (which is only a crude approximation because, as was pointed out earlier, the process has not terminated), we have

$$\frac{2}{3} = \frac{\beta + \alpha}{2\beta + \alpha}$$

hence $\beta = \alpha$. Thus, for an asymptote of $\frac{2}{3}$, the two-parameter model reduces to the one-parameter model. In approaching the present model, we required $\beta > \alpha$ in order to decrease the probability of exactly zero errors and to increase the expected number of alternations. But it is easy to show that for the present model $\beta > \alpha$ implies $p(\infty) < \frac{2}{3}$.

Let

$$Y = p(\infty)$$

From Equation 5.43

$$Y = \frac{\beta + \alpha}{2\beta + \alpha}$$

and

$$2\beta Y - \beta = \alpha - \alpha Y$$

$$\frac{2Y - 1}{1 - Y} = \frac{\alpha}{\beta}$$

but $\beta > \alpha$ implies

$$1 - Y > 2Y - 1$$

By adding 1 to both sides and dividing by 2, we get

$$1 - \frac{Y}{2} > Y$$

or

$$1 > \frac{3}{2} Y$$

Now multiplying both sides by $\frac{2}{3}$ gives

$$\frac{2}{3} > Y$$

or

$$\frac{2}{3} > p(\infty)$$

Thus we have proven the restriction.

Furthermore the converse, that is, $p(\infty) < \frac{2}{3}$ implies $\beta > \alpha$ is also true. And in a similar fashion it is easy to demonstrate that $\alpha > \beta$ implies and is implied by $p(\infty) > \frac{2}{3}$ for the two-parameter model.

Although it is not possible to demonstrate that the asymptotic probability of a correct response in the actual experiment is equal to or greater than $\frac{2}{3}$, there is no evidence in the data that it is sufficiently less than $\frac{2}{3}$, so that β would be appreciably greater than α. The analysis in connection with the one-parameter model suggests that this asymptote is at least equal to $\frac{2}{3}$. Thus the conclusion to draw is that the desired asymptotic properties and the desired properties of the distribution of errors and the distribution of alternations are incompatible in the model as it now stands. For this reason, further analysis of the two-parameter model does not seem fruitful.

Although we shall not consider the two-parameter model further, it may be that there are experiments to which this case might apply. For that reason, we shall present without proof a few additional results for this model:

$$\Pi_1 = \frac{\alpha\beta^2}{(\alpha + \beta)^2} \tag{5.44}$$

$$\Pi_2 = \frac{\alpha\beta^2}{(\alpha + \beta)^3} [\beta^2 + (\alpha + \beta) - (\alpha + \beta)^2] \tag{5.55}$$

$$\phi_k = \frac{\alpha\beta^k}{(\alpha + \beta)^{k+1}} \tag{5.46}$$

$$E(A) = \frac{\beta}{\alpha} \tag{5.47}$$

$$\sigma^2(A) = \frac{\beta}{\alpha}(1 + \frac{\beta}{\alpha}) \tag{5.48}$$

5.4 Summary

In this chapter, we have examined two special cases of the conflict model. With the one-parameter model, it was possible to derive analytically several interesting properties. Each of these properties was compared with statistics from the Moderate condition for an indication of the fit of the model to the data. Where properties could not be obtained mathematically, Monte

Carlo methods generated theoretical values that were then checked with the experimental results.

The one-parameter model closely described the Moderate condition with respect to the trial-by-trial trend in the probability of a correct response and the variance of the distribution of errors. This model overestimated the probability of completely correct sequence and overestimated the rate of becoming consistent. In general, the model predicted more consistency than was observed, as comparing the theoretical and observed distributions of alternations illustrated. The analysis of the one-parameter model can be regarded as highly encouraging; with these highly restrictive assumptions and with only one quantity estimated from the observations, it was possible to describe some interesting and important aspects of the data.

Data from the Extreme condition was not used to test either the one- or the two-parameter model, because it was immediately apparent that the process described by these special cases was not the process occurring in the Extreme condition. Both of these models lead to a monotonic decreasing trend in the probability of a correct response, a situation that is clearly not the case in the Extreme condition. It should be noted that in the course of this analysis we have shown a sufficient condition for $p(n)$ to be monotone decreasing, namely, that $\alpha = \epsilon$ and $\beta = \gamma$.

Of the three possible two-parameter models, one was chosen for study. This model fulfilled the requirement that the probability of movement to a temporary state was greater than the probability of movement to an absorbing state. The major difficulty with the one-parameter model seemed to be a too rapid rate of absorption; hence, the choice of a two-parameter model attempted to compensate for this deficiency by imposing the restriction: $\beta > \alpha$, $\gamma > \epsilon$. The two-parameter model did not fit the data as well as the first model. And the final analysis demonstrated that the two desired properties were incompatible. The third restriction $\beta > \alpha$ implied an asymptotic probability of a correct response less than $\frac{2}{3}$.

The approach to the one- and two-parameter models provided a program for the analysis of the remaining models. In addition, this chapter introduced most of the mathematical tools to be used in the remaining investigation. Chapter 6 will consider the same properties and utilize similar analytic methods in examining a three-parameter model.

NOTES

1. See p. 35.
2. An alternation occurs when a subject gives one response on trial n and a different response on trial n + 1, for example, correct on n and incorrect on n + 1.

3. $E(\sum X_n) = \sum E(X_n)$.

4. For the form of inductive proofs, see Polya (1954), pp. 108-116, especially pp. 110-111.

5. It should be noted that some tests of the model can be made without knowledge of the estimate of α. The order of presentation here is chosen for convenient exposition.

6. Thus the degree of fit of the theoretical to the observed mean curve in Figure 5.1 represents a successful prediction of the model.

7. More rigorously, this would be: let $\Pi_k = \Pr\left\{\sum_{n=1}^{\infty} \sum_{i=1}^{M} (1 - X_{in}) = k\right\}$ in terms of the definition of the random variable X_{in} given in Chapter 3.

8. The limits are based on the estimate of $\hat{\alpha} = .07$ and apply to the Moderate condition only.

9. The model assumes that he cannot be in S_4 on trial 1.

10. The first set in Equations 5.31 is an obvious contradiction of the requirement. The second contradicts Equations 5.32 in the following ways: If $\beta > \alpha$ and $\beta = \epsilon$, then $\epsilon > \gamma$ because $\gamma = \alpha$, but we require $\gamma > \epsilon$, so that this does not satisfy the required condition.

11. This development is identical with that discussed following Equations 5.2.

Chapter 6

THE THREE-PARAMETER MODEL

6.1 Introduction

In this chapter, we shall examine a three-parameter model. First, we shall justify the particular case we have chosen; then we shall derive several properties of this case; we shall next test the fit of the model to the data; and, finally, we shall consider some of the problems resulting from the tests of goodness of fit. Data from both the Moderate and the Extreme condition will be examined in this chapter, since the difficulties in the previous analysis with respect to the Extreme condition no longer apply.

This model has three parameters that must be estimated from the data. Since we are limited in what we can derive, the order of analysis in this chapter will differ from that of Chapter 5. There we derived a property and then examined the data for that property before proceeding to the next derivation. For reasons that will be apparent in the next section, we shall do all the mathematical analysis before considering the data. In the next section, we shall consider properties of the model and problems of estimation of the parameters. Section 6.3 will examine data from the Moderate condition and Section 6.4 data from the Extreme condition. In Section 6.5, we shall reconsider the Moderate condition in light of the results of analyzing the Extreme data.

6.2 Properties of the Three-Parameter Model

With this model, we again have a choice concerning which parameters to equate. If we use the notation of Section 4.2, the alternative sets of parameters are

$$\left.\begin{array}{ll}
1. & \alpha = \beta,\ \gamma,\ \epsilon \\
2. & \alpha = \gamma,\ \beta,\ \epsilon \\
3. & \alpha = \epsilon,\ \beta,\ \gamma \\
4. & \alpha,\ \beta = \gamma,\ \epsilon \\
5. & \alpha,\ \beta = \epsilon,\ \gamma \\
6. & \alpha,\ \beta,\ \gamma = \epsilon
\end{array}\right\} \tag{6.1}$$

Alternatives 1 and 6 are eliminated because we desire, for reasons given in the previous chapter, $\beta > \alpha$ and $\gamma > \epsilon$. In connection with alternative 5, these restrictions lead to the inequality

$$\epsilon > \alpha$$

But the data show that more subjects become consistently correct than become consistently incorrect. We regard consistent sequences as indicating absorption, correct sequences in S_1 and incorrect in S_4. Furthermore, α and ϵ may be considered rates of absorption in S_1 and S_4, respectively. In view of the data, it does not seem reasonable to assume that the rate of absorption into S_4 is greater than the rate into S_1. If we made this assumption, we should have to build into the model some additional features to compensate for these rate differences in order to yield the proper distribution of subjects in S_1 and S_4. Rather than $\epsilon > \alpha$, a desirable, although not strictly necessary assumption, is

$$\alpha > \epsilon$$

In addition to eliminating alternative 5, this restriction also rules out alternative 3.

Hence two alternatives remain. The analysis up to this point indicates that either set would be reasonable to investigate. Of course, it would be desirable to consider both alternative models; ideally, we should investigate all the models resulting from these alternative sets of assumptions. Limitations of time preclude such an endeavor and require us to choose one set for a thorough examination. We have chosen alternative 4, viz.,

$$\alpha,\ \beta = \gamma,\ \epsilon \tag{6.2}$$

First we write the transition equations for this model. The expressions analogous to Equations 5.1 and 5.34 are

$$\left.\begin{aligned} p_1(n+1) &= p_1(n) + \alpha p_2(n) \\ p_2(n+1) &= (1-\alpha-\beta)p_2(n) + \beta p_3(n) \\ p_3(n+1) &= (1-\beta-\epsilon)p_3(n) + \beta p_2(n) \\ p_4(n+1) &= p_4(n) + \epsilon p_3(n) \end{aligned}\right\} \tag{6.3}$$

To illustrate these transitions, we modify Figure 5.2 as shown in Figure 6.1.

To estimate the parameters of this model, it is obvious that we require three independent equations. For the previous models, we derived expressions for $E(\sum X_n)$, Π_0, and $p(\infty)$ as func-

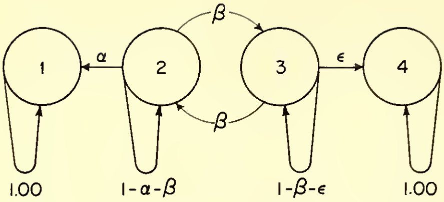


Figure 6.1. States and transitions for the three-parameter model.

tions of the parameters. The same procedure seems to be in order for this model; but here we do not have the same properties of symmetry we had earlier. We have found no straightforward way to derive an expression for p(n); hence, we cannot derive $E(\sum X_n)$ and must resort to alternative means for deriving $p(\infty)$. Although a formal solution exists for p(n), and therefore $\sum p(n)$, the labor involved in utilizing this solution to estimate the parameters is prohibitive.

In the remainder of this section, we shall demonstrate why the procedure used in the one- and two-parameter models to derive an expression for p(n) does not work in this case. Second, we shall present a formal solution for p(n) and point out the practical difficulty of applying this formal solution. Following this discussion, some important derivations that can be made will be presented; in the next sections we shall estimate the parameters and compare the model with the data from both the Moderate and Extreme condition.

To derive p(n) in Sections 5.2 and 5.3, we defined $\delta(n)$ as $p_2(n) - p_3(n)$, and we were able, by subtraction, to obtain a simple recurrence relation:

$$\delta(n + 1) = C\ \delta(n) \tag{6.4}$$

where C is a constant term involving the parameters. The solution of this system, C^n, led to a recurrence relation for $p(n + 1)$ involving only p(n) and $\delta(n)$ which, in turn, had a straightforward solution. If we let $\delta(n) = p_2(n) - p_3(n)$ in the present model, we do not obtain an expression of the form of Equation 6.4.

$$\begin{aligned}\delta(n + 1) &= (1 - \alpha - \beta)p_2(n) + \beta p_3(n) - (1 - \beta - \epsilon)p_3(n) - \beta p_2(n)\\ &= (1 - \alpha - 2\beta)p_2(n) - (1 - 2\beta - \epsilon)p_3(n)\\ &= (1 - 2\beta)\delta(n) - \alpha p_2(n) + \epsilon p_3(n) \qquad (6.5)\end{aligned}$$

Equation 6.5 is not a simple linear recurrence relation because it involves terms other than $\delta(n)$ dependent on n, and no solution for this system was found. Attempts to use devices similar to $\delta(n)$ also failed to yield an explicit expression for p(n).

Another approach to the problem involves matrix theory. Let P_n be the vector $[p_1(n), p_2(n), p_3(n), p_4(n)]$ and A be the matrix of transition probabilities

$$A = \begin{bmatrix} 1 & 0 & 0 & 0 \\ \alpha & 1 - \alpha - \beta & \beta & 0 \\ 0 & \beta & 1 - \beta - \epsilon & \epsilon \\ 0 & 0 & 0 & 1 \end{bmatrix}$$

The rows of this matrix represent states of origin and the columns, states of destination. Thus the diagonal elements are the probabilities of remaining in the same state from trial n to trial n + 1; similarly, the off-diagonal elements are the probabilities of moving from one given state to another on the next trial.

The desired relation is then

$$P_n = P_0 A^n \tag{6.6}$$

where P_0 is the vector (0, 1, 0, 0) given by our assumption concerning the initial state of the system. From this relation it is possible to find an expression for p(n) for any given value of n by computing the appropriate power of the matrix A, multiplying it by P_0, then selecting the appropriate elements of the vector P_n and adding these elements. This method, however, does not provide a general expression for p(n). Since we require an expression for $\sum_{n=1}^{N} p(n)$, it would be necessary to evaluate Equation 6.6 for each value of n, select the first two elements of each vector P_n, and then form a sum of the resulting expressions. The magnitude of such a task is readily appreciated if we merely compute P_2.

Squaring the matrix A yields

$$A = \begin{bmatrix} 1 & 0 & 0 & 0 \\ \alpha + \alpha(1 - \alpha - \beta) & (1 - \alpha - \beta)^2 + \beta^2 & \beta(2 - 2\beta - \alpha - \epsilon) & \beta\epsilon \\ \alpha\beta & \beta(2 - 2\beta - \alpha - \epsilon) & \beta^2 + (1 - \beta - \epsilon)^2 & \epsilon(1 - \beta - \epsilon) + \epsilon \\ 0 & 0 & 0 & 1 \end{bmatrix}$$

and

$$P_2 = P_0 A^2$$

hence,

$$P_2 = [\alpha + \alpha(1 - \alpha - \beta),\ (1 - \alpha - \beta)^2 + \beta^2,\ \beta(2 - 2\beta - \alpha - \epsilon),\ \beta\epsilon]$$

Remembering that $p(n) = p_1(n) + p_2(n)$, and adding the elements of the vector, we have

$$p(2) = \alpha + \alpha(1 - \alpha - \beta) + (1 - \alpha - \beta)^2 + \beta^2$$

Because for $n > 2$, powers of ϵ are involved in the expression, the complexity of a sum of such terms becomes quite clear. Therefore, this approach did not appear fruitful in view of the limited resources at hand.

Since we are unable to find an expression for $p(n)$, we must investigate other statistics that we can express as functions of α, β, and ϵ. The first property to examine is the distribution of errors. As in Section 5.2, let

$$\Pi_k = \Pr\{\text{precisely } k \text{ errors}\}$$

At this point, we shall derive expressions for five points of the distribution of errors.

For $k = 0$, we must sum over all sequences of states of the form

$$\underbrace{2----------2}_{i+1}1----------$$

This desired quantity[1] is

$$\sum_{i=0}^{\infty} (1 - \alpha - \beta)^i \alpha$$

Evaluating this sum using Equation 5.12, we have

$$\Pi_0 = \frac{\alpha}{\alpha + \beta} \tag{6.7}$$

The form of a sequence leading to exactly one error is

$$\underbrace{2----------2}_{i+1}3\underbrace{2----------2}_{j+1}1----------$$

The probability of a sequence of this form is given by

$$\alpha\beta^2 \left[\sum_{i=0}^{\infty} (1 - \alpha - \beta)^i\right] \left[\sum_{j=0}^{\infty} (1 - \alpha - \beta)^j\right]$$

which when evaluated leads to

$$\Pi_1 = \frac{\alpha\beta^2}{(\alpha + \beta)^2} \tag{6.8}$$

The parameter ϵ enters in when we consider the probability of precisely two errors. Two forms of sequences result in exactly two errors. These forms are

$$\text{a.}\quad 2\underbrace{\text{----------}2}_{i+1}33\underbrace{2\text{----------}}_{j+1}21\text{-}$$

and

$$\text{b.}\quad 2\underbrace{\text{----------}2}_{r+1}3\underbrace{2\text{----------}2}_{s+1}3\underbrace{2\text{----------}}_{t+1}21\text{-}$$

The probability of remaining in S_3 from n to n + 1 is $(1 - \beta - \epsilon)$, so that the probability of any sequence like a is

$$\frac{\alpha\beta^2(1 - \beta - \epsilon)}{(\alpha + \beta)^2}$$

On the other hand, the probability of a sequence like b is

$$\frac{\alpha\beta^4}{(\alpha + \beta)^3}$$

because there are four transitions between S_2 and S_3 (probability $= \beta$), one transition from S_2 to S_1 (probability $= \alpha$), and three sequences in S_2 (probability $= \frac{1}{\alpha + \beta}$). Now the probability of two errors is the probability of either sequence a or sequence b. Hence,

$$\Pi_2 = \frac{\alpha\beta^2(1 - \beta - \epsilon)}{(\alpha + \beta)^2} + \frac{\alpha\beta^4}{(\alpha + \beta)^3} \tag{6.9}$$

We pointed out in Section 5.2 that it is difficult to pursue the derivation of Π_k for much beyond $k = 2$ because of the increasing number of sequences leading to exactly k errors. If we look at the other tail of the distribution, however, we can compute a few additional points. Only one form of sequence leads to exactly 36 errors, viz:

23----------34----------

Similarly, the forms

23----------323----------34----------

and

223----------34----------

lead to 35 errors. Since the probability of a subsequence 3-----3 is $\frac{1}{\beta + \epsilon}$ and the probability of transitions 2 to 3 and 3 to 4 are β and ϵ, respectively,

$$\Pi_{36} = \frac{\beta\epsilon}{\beta + \epsilon} \tag{6.10}$$

and

$$\Pi_{35} = \frac{\beta^3\epsilon}{(\beta + \epsilon)^2} + \frac{\beta\epsilon(1 - \alpha - \beta)}{\beta + \epsilon} \tag{6.11}$$

This completes the derivations.

Although we have derived five equations, all but the equation for Π_0 are of little use in estimating the parameters. The observed values of Π_{35} and Π_{36} are both 0, so that only trivial solutions or solutions lying outside the acceptable interval exist.[2] In addition, the observed values of Π_1 and Π_2 are small and thus are extremely sensitive to measurement error. For example, there are three subjects who make a single error in the course of 36 trials, so that the observed value of $\Pi_1 = .091$. If one response of a subject who made either zero, one, or two errors was recorded incorrectly, the estimate of Π_1 could be either .06 or .121. The values of the parameters would then be considerably altered on the basis of incorrectly scoring just one response. To illustrate this, let us estimate α and β (since ϵ does not enter those equations), using Equations 6.7 and 6.8, assuming $\Pi_0 = .3$, and $\Pi_1 = .06$, .09, and .12, respectively.[3]

For $\Pi_1 = .06$:

$$\Pi_0 = .3 = \frac{\alpha}{\alpha + \beta}$$

$$\Pi_1 = .06 = \frac{\alpha\beta^2}{(\alpha + \beta)^2}$$

From the first equation, $\beta = 7/3\,\alpha$, and substituting in the second, we have

$$.06 = \frac{\frac{49}{9}\alpha^3}{\frac{100}{9}\alpha^2}$$

and

$$\alpha \mathrel{\hat{=}} .12$$

$$\beta \mathrel{\hat{=}} .28$$

For $\Pi_1 = .09$:

$$.09 = \frac{49}{100}\,\alpha$$

so that

$$\alpha \mathrel{\hat{=}} .18 \text{ and } \beta \mathrel{\hat{=}} .42$$

Finally, for $\Pi_1 = .12$:

$$\alpha \mathrel{\hat{=}} .24 \text{ and } \beta \mathrel{\hat{=}} .56$$

The example just given illustrates why a statistic such as Π_1 is a poor statistic from which to estimate the values of α, β, and ϵ. Thus, the statistic we should like to use for estimation of the parameters should not be too sensitive to small errors of measurement. Furthermore, such statistics should contain more information about the data as a whole than does a statistic like Π_1.[4]

We turn next to the distribution of alternations in an effort to derive additional formulas for estimating the parameters. As in Section 5.2, let $\phi_k = \Pr\{A = k\}$, that is, the probability of precisely k alternations. It will be remembered that an alternation refers to a change of response from one trial to the next rather than a movement between states; that is, only movements between S_2 and S_3 produce alternations. Following the procedure used to derive the distribution function for the distribution of alternations in the one-parameter model, we shall write out the sequences of states that lead to k alternations for the first few values of k. The function here is more complex than in the previous case, but we arrive at an expression for ϕ_k in much the same manner as before. In addition to writing out these sequences, however, we shall write out the number of times each "transition event" occurs as well as the probability of this event. We use the term "transition event" rather than simply transition, because we wish to include subsequences of the form, 2-----2, 3-----3, which are not single transitions, but for which we can assign probabilities. (The probability of any sequence of the form 2------2, where there are i + 1 2's, is given by $\sum_{i=0}^{\infty}(1 - \alpha - \beta)^{i} = \frac{1}{\alpha + \beta}$ from the computations in connection with Equation 6.7. Similarly, the probability of any sequence of the form 3-----3, is given by $\frac{1}{\beta + \epsilon}$.) Hence, we have Table 6.1.

Table 6.1. Number of Occurrences of Each Transition Event in Sequences Leading to k Alternations for a Few Values of k

Number of Alternations		Transition Event					
		2-2	2-3	3-2	2-1	3-3	3-4
k	Sequence						
	(Probability)	$\left(\frac{1}{\alpha+\beta}\right)$	(β)	(β)	(α)	$\left(\frac{1}{\beta+\epsilon}\right)$	(ϵ)
0	2---21------------------------	1	0	0	1	0	0
1	2---23---34-------------------	1	1	0	0	1	1
2	2---23---32---21--------------	2	1	1	1	1	0
3	2---23---32---23---34---------	2	2	1	0	2	1
4	2---23---32---23---32---21---	3	2	2	1	2	0

The probability of any sequence of a given form is the product of the probabilities of the transition events in that sequence. From Table 6.1, we can now write the probability of any sequence listed, and hence of the number of alternations in that sequence. For example, for k = 3, the transition event 2-2 occurs twice; the probability of this event occurring twice is $\left(\frac{1}{\alpha+\beta}\right)\left(\frac{1}{\alpha+\beta}\right)$. Thus, the cell entry in the table is the power to which the probability listed at the top of the column should be raised for the value of k to which the row refers. Using the table in this way, we can write

$$\Pr(A = 3) = \left(\frac{1}{\alpha+\beta}\right)^2 \beta^2 \beta \alpha^0 \left(\frac{1}{\beta+\epsilon}\right)^2 \epsilon$$

$$= \frac{\beta^3\epsilon}{(\alpha+\beta)^2(\beta+\epsilon)^2}$$

To find a general expression for the probability of k alternations, we should first note that when k is even, absorption takes place in S_1, and the transition 3-4 does not occur. Similarly, when k is odd, absorption is in S_4, and the transition 2-1 does not occur. In the former case, ϵ will not appear in the numerator of the expression, whereas in the latter α will be absent from the numerator. This implies that there will not be an expression that holds for all values of k but rather an expression for even values of k and a separate expression when k is odd. Now let us return to Table 6.1 and write in Table 6.2 the general term for each cell entry for even and odd values of k separately.

Table 6.2. Number of Occurrences of Each Transition Event in Sequences Leading to k Alternations as a Function of k

	Transition Event					
	2-2	2-3	3-2	2-1	3-3	3-4
Even Values of k	$\frac{k}{2} + 1$	$\frac{k}{2}$	$\frac{k}{2}$	1	$\frac{k}{2}$	0
Odd Values of k	$\frac{k+1}{2}$	$\frac{k+1}{2}$	$\frac{k-1}{2}$	0	$\frac{k+1}{2}$	1
(Probability)	$\left(\frac{1}{\alpha+\beta}\right)$	(β)	(β)	(α)	$\left(\frac{1}{\beta+\epsilon}\right)$	(ϵ)

The reader can verify the entries in Table 6.2 for the first few values of k by substituting the value of k and then comparing the result with Table 6.1. That these entries are true in general can be shown by mathematical induction. By taking the product of the probabilities of these events to the power shown in Table 6.2, we arrive at the desired expression for ϕ_k:

$$\phi_k = \left\{ \begin{array}{ll} \dfrac{\alpha\beta^k}{(\alpha+\beta)^{\frac{k}{2}+1}(\beta+\epsilon)^{\frac{k}{2}}} & \text{for k even} \\ \\ \dfrac{\epsilon\beta^k}{(\alpha+\beta)^{\frac{k}{2}+\frac{1}{2}}(\beta+\epsilon)^{\frac{k}{2}+\frac{1}{2}}} & \text{for k odd} \end{array} \right\} \tag{6.12}$$

The first problem is to show that ϕ_k is a distribution function, that is, that it sums to unity.

Let

$$Z = \frac{\beta^2}{(\alpha+\beta)(\epsilon+\beta)} \tag{6.13}$$

then

$$\phi_k = \left\{ \begin{array}{ll} \dfrac{\alpha}{\alpha+\beta} Z^{\frac{k}{2}} & \text{for k even} \\ \\ \dfrac{\epsilon}{(\alpha+\beta)^{\frac{1}{2}}(\beta+\epsilon)^{\frac{1}{2}}} Z^{\frac{k}{2}} & \text{for k odd} \end{array} \right\} \tag{6.14}$$

Summing this over all values of k, we have

$$\sum_{k=0}^{\infty} \phi_k = \frac{a}{a+\beta}(1 + Z + Z^2 + Z^3 + \cdots) + \frac{\epsilon}{(a+\beta)^{\frac{1}{2}}(\beta+\epsilon)^{\frac{1}{2}}}(Z^{\frac{1}{2}} + Z^{\frac{3}{2}} + Z^{\frac{5}{2}} + \cdots) \tag{6.15}$$

Factoring a $Z^{\frac{1}{2}}$ from the second term of the right-hand member gives

$$\sum_{k=0}^{\infty} \phi_k = \frac{a}{a+\beta}(1 + Z + Z^2 + \cdots) + \frac{\beta\epsilon}{(a+\beta)(\beta+\epsilon)}(1 + Z + Z^2 + \cdots)$$

But the series in parentheses is the familiar $\sum_{i=0}^{\infty} Z^i$; therefore, from Equation 5.12 we have

$$\sum_{k=0}^{\infty} \phi_k = \frac{1}{1-Z}\left[\frac{a}{a+\beta} + \frac{\beta\epsilon}{(a+\beta)(\beta+\epsilon)}\right]$$

$$= \frac{1}{1-Z}\left[\frac{a(\beta+\epsilon)+\beta\epsilon}{(a+\beta)(\beta+\epsilon)}\right] \tag{6.16}$$

Substituting for Z and simplifying give

$$\sum_{k=0}^{\infty} \phi_k = \frac{a(\beta+\epsilon)+\beta\epsilon}{(a+\beta)(\beta+\epsilon)-\beta^2} = \frac{a\epsilon + a\beta + \beta\epsilon}{a\epsilon + a\beta + \beta\epsilon} = 1$$

Therefore, we have shown that ϕ_k is a distribution function.

We have already commented that an even number of alternations leads to absorption in S_1. But when the asymptote is reached, the proportion of subjects absorbed in S_1 is the proportion responding correctly; that is, the total probability of absorption in S_1 is the same as the asymptotic probability of a correct response. Moreover, the total probability of absorption is S_1 is identical with the total probability of an even number of alternations. Thus we have the following relation:

$$p(\infty) = \sum_{k \text{ even}}^{\infty} \phi_k, \quad k = 0, 2, 4, \cdots \tag{6.17}$$

But we have already evaluated the right-hand member of this expression. It is simply the first product of the right-hand member of Equation 6.15. That is,

$$\sum_{k\ \text{even}}^{\infty} \phi_k = \frac{1}{1 - Z}\left(\frac{\alpha}{\alpha + \beta}\right)$$

Substituting for Z and simplifying lead to

$$p(\infty) = \sum_{k\ \text{even}}^{\infty} \phi_k = \frac{\alpha(\beta + \epsilon)}{\alpha(\beta + \epsilon) + \beta\epsilon} \tag{6.18}$$

Equation 6.18 gives us a second equation for estimating α, β, and ϵ. The remaining statistics for which we can derive expressions in terms of the parameters are the mean and variance of the distribution of alternations. Although we shall proceed to derive these expressions, they are of no assistance in estimating the parameters because once the asymptote and Π_0 are fixed, the values of the mean and the variance of the distribution of alternations are determined. For example, it can easily be shown, after we have derived an expression for E(A), that

$$E(A) = \frac{1}{\Pi_0}\left[2p(\infty) - \Pi_0 p(\infty) - \Pi_0\right]$$

Here it is necessary to prove that

$$E(A) = \frac{\beta}{\alpha} + \frac{\beta^2}{\alpha}\,\frac{\alpha - \epsilon}{\alpha\beta + \alpha\epsilon + \beta\epsilon}$$

and also to derive an expression for $\sigma^2(A)$.

The mean of the distribution of alternations is given by Equation 5.24. Substituting the expression for ϕ_k (using the form of Equation 6.14) derived for the three-parameter model in this equation, we have

$$E(A) = \sum_{k=0}^{\infty} k\phi_k = \frac{\alpha}{\alpha + \beta}\sum_{k\ \text{even}}^{\infty} kZ^{\frac{k}{2}} + \frac{\epsilon}{(\alpha + \beta)^{\frac{1}{2}}(\beta + \epsilon)^{\frac{1}{2}}}\sum_{k\ \text{odd}}^{\infty} kZ^{\frac{k}{2}} \tag{6.19}$$

Writing out the terms of both summations and factoring $Z^{\frac{1}{2}}$ from the second series, we obtain

$$E(A) = \frac{\alpha}{\alpha + \beta}(2Z + 4Z^2 + 6Z^3 + \cdots) + \frac{\beta\epsilon}{(\alpha + \beta)(\beta + \epsilon)}(1 + 3Z + 5Z^2 + 7Z^3 \cdots)$$

Some simple algebraic manipulation will convert this relation into some series with which we are already familiar. First we factor a 2 from the first series; then we break the second into two separate series, with the result

$$E(A) = \frac{2\alpha}{\alpha + \beta}(Z + 2Z^2 + 3Z^3 + \cdots)$$

$$+ \frac{\beta\epsilon}{(\alpha + \beta)(\beta + \epsilon)}[(1 + Z + Z^2 + \cdots) + 2(Z + 2Z^2 + 3Z^3 + \cdots)]$$

But the first and the third series are both $\sum_{k=0}^{\infty} kZ^k$, while the second is $\sum_{k=0}^{\infty} Z^k$. Applying Equation 5.25 to the former and Equation 5.12 the latter gives

$$E(A) = \frac{2\alpha}{\alpha + \beta}\,\frac{Z}{(1 - Z)^2} + \frac{\beta\epsilon}{(\alpha + \beta)(\beta + \epsilon)}\left[\frac{1}{1 - Z} + \frac{2Z}{(1 - Z)^2}\right]$$

$$= \frac{2\alpha}{\alpha + \beta}\,\frac{Z}{(1 - Z)^2} + \frac{\beta\epsilon}{(\alpha + \beta)(\beta + \epsilon)}\,\frac{1 + Z}{(1 - Z)^2} \qquad (6.20)$$

By using Equation 6.13, it is possible to obtain an expression for E(A) solely in terms of the parameters α, β, and ϵ. However, to obtain a simplified expression requires much tedious algebra which we shall omit. The final result of this algebra is

$$E(A) = \frac{\beta}{\alpha} + \frac{\beta^2}{\alpha}\,\frac{\alpha - \epsilon}{\alpha\beta + \alpha\epsilon + \beta\epsilon} \qquad (6.21)$$

For the variance of the distribution of alternations, we require $\sum_{k=0}^{\infty} k^2\phi_k$. Analogous to Equation 6.19, we have

$$E(A^2) = \sum_{k=0}^{\infty} k^2\phi_k = \frac{\alpha}{\alpha + \beta}\sum_{k\ \mathrm{even}}^{\infty} k^2 Z^{\frac{k}{2}} + \frac{\epsilon}{(\alpha + \beta)^{\frac{1}{2}}(\beta + \epsilon)^{\frac{1}{2}}}\sum_{k\ \mathrm{odd}}^{\infty} k^2 Z^{\frac{k}{2}}$$

$$(6.22)$$

where Z again equals $\frac{\beta^2}{(\alpha + \beta)(\beta + \epsilon)}$. Writing out the terms of both summations and factoring a 4 from the first and $Z^{\frac{1}{2}}$ from the second series, we obtain

$$(A)^2 = \frac{4\alpha}{\alpha+\beta}(Z + 4Z^2 + 9Z^3 + 16Z^4 + \cdots) + \frac{\beta\epsilon}{(\alpha+\beta)(\beta+\epsilon)}(1 + 9Z + 25Z^2 + 49Z^3 + \cdots)$$

The first series we have encountered earlier. It is $\sum_{k=0}^{\infty} k^2 Z^k$, whose value is given by Equation 5.29. Furthermore, the second series can be written as three separate series with which we are familiar. That is,

$$(1 + 9Z + 25Z^2 + \cdots) = 4\sum_{k=0}^{\infty} k^2 Z^k + 4\sum_{k=0}^{\infty} kZ^k + \sum_{k=0}^{\infty} Z^k$$

It is easy to verify that any term of this series is given by $4k^2 Z^k + 4kZ^k + Z^k$. For $k = 3$, for example,

$$49Z^3 = 4(9)Z^3 + 4(3)Z^3 + Z^3$$

To evaluate Equation 6.22, we use Equations 5.29, 5.25, and 5.12; we find

$$E(A^2) = \frac{4\alpha}{\alpha+\beta}\left[\frac{2Z^2}{(1-Z)^3} + \frac{Z}{(1-Z)^2}\right]$$

$$+ \frac{\beta\epsilon}{(\alpha+\beta)(\beta+\epsilon)}\left\{4\left[\frac{2Z^2}{(1-Z)^3} + \frac{Z}{(1-Z)^2}\right] + \frac{4Z}{(1-Z)^2} + \frac{1}{1-Z}\right\}$$

which simplifies to

$$E(A^2) = \frac{1}{(1-Z)^3(\alpha+\beta)}\left[4\alpha(Z^2 + Z) + \frac{\beta\epsilon}{\beta+\epsilon}(Z^2 + 6Z + 1)\right] \qquad (6.23)$$

We obtain the variance by substituting Equations 6.20 and 6.23 in Equation 5.27.

Thus,

$$\sigma^2(A) = \frac{1}{(1-Z)^3(\alpha+\beta)}\left[4\alpha(Z^2 + Z) + \frac{\beta\epsilon}{\beta+\epsilon}(Z^2 + 6Z + 1)\right]$$

$$- \left[\frac{2\alpha}{\alpha+\beta}\,\frac{Z}{(1-Z)^2} + \frac{\beta\epsilon}{(\alpha+\beta)(\beta+\epsilon)}\,\frac{1+Z}{(1-Z)^2}\right]^2 \qquad (6.24)$$

Further simplification of this expression requires considerable effort. As we are not able to use Equation 6.24 in estimating parameters, the present form is adequate. Hence, we shall not attempt to simplify it further.

We have thus completed all the derivations we can at present accomplish for the three-parameter model. Unfortunately, we do not have three independent equations to solve for α, β, and ϵ. Using the data and Equations 6.7 and 6.18, the expressions for Π_0 and $p(\infty)$, respectively, we can find β and ϵ in terms of α. To find α, we use a trial-and-error method. This method consists of assuming a value of α, running a sample of Monte Carlo subjects, computing the mean number of errors for this Monte Carlo sample, and comparing this mean with the observed mean. Several values of α were assumed, and several Monte Carlo samples all based on the same set of random numbers were examined the parameter values that produced a mean nearest to the observed were the values chosen. Without additional refinement, this method does not lead to the best-fitting set of parameters. But until we have additional tools,[5] we must work with this approximate solution.

6.3 Analysis of the Moderate Condition

To estimate the parameters for the Moderate condition, we first find β and ϵ in terms of α. In connection with the one-parameter model, we used the following estimates:

$$\hat{\Pi}_0 = \frac{3}{10}$$

$$\hat{p}(\infty) = \frac{2}{3}$$

For the first test of the present model, we shall use these again. Substituting these values in Equations 6.7 and 6.18, we obtain the relations

$$\left.\begin{aligned} \beta &= \frac{7}{3}\alpha \\ \epsilon &= \frac{7}{11}\alpha \end{aligned}\right\} \qquad (6.25)$$

Using Equations 6.25, we then applied the approximation technique. In principle, we could have investigated any value of α such that the restriction

$$0 < \frac{7}{11}\alpha < \frac{7}{3}\alpha < 1$$

holds.[6] However, we were guided in the choice of α by the examination of the one-parameter model and thus investigated values of α in the neighborhood of .07. The set of parameters that gave the closest approximation to the observed mean was

$$\left.\begin{aligned}\hat{\alpha} &= .05\\ \hat{\beta} &= \frac{7}{3}\alpha = .117\\ \hat{\epsilon} &= \frac{7}{11}\alpha = .032\end{aligned}\right\} \qquad (6.26)$$

which yielded a mean of 12.0 for a single sample, whereas the observed mean was 10.8. Although this is not as close as desired, it was decided to study further this set of parameters, recognizing that some of the discrepancy between the model and the data could be eliminated with a better-fitting set of parameters. The purpose of this analysis was to determine what direction to take in finding more appropriate parameter values.

Although we do not have an expression for p(n), it is still possible to obtain numerically the theoretical mean curve. Repeated substitution in the transition equations (Equations 6.3) yields values for $p_j(n)$ for all n. From this, the mean curve is the sum of $p_1(n) + p_2(n)$ for each value of n. The theoretical curve thus obtained is shown in Figure 6.2, where the observed curve is also presented.

The comparison in Figure 6.2 illustrates several facts about this model. First of all, it is clear that this set of parameters

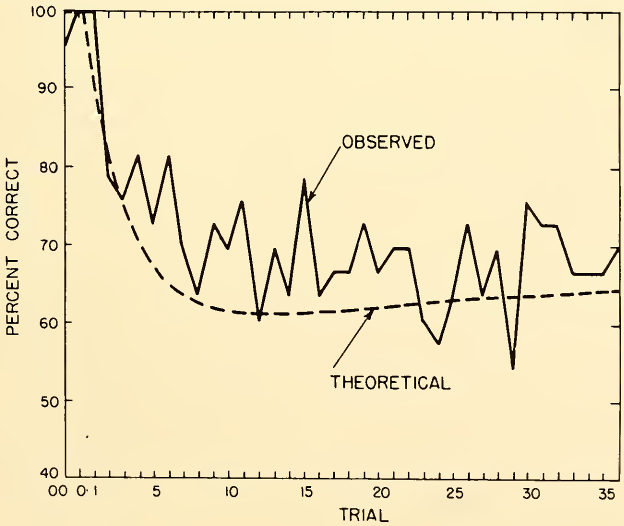


Figure 6.2. Comparison of the theoretical and observed mean curves. (Moderate condition; three-parameter model: $\alpha = .05$, $\beta = .117$, $\epsilon = .032$).

does not yield a mean curve which fits the observed very closely. But this fact should not obscure other features that the figure shows. The theoretical p(n) for this model is a closer approximation to the function desired than was that computed from the two-parameter model (shown in Figure 5.3). In the second place, the mean curve for this model is not a monotonic function of n, but rather reaches a minimum lower than the asymptotic value and then slowly rises toward this asymptote. Both the value of this minimum and the rate at which the curve rises are, of course, functions of the parameter values. It is with respect to the property of being monotonic that the three-parameter model differs from the models already discussed; the fact that p(n) need not be a monotonic function of n has important consequences for the fit of the model to the data. For example, to increase the expected number of alternations, we require a large β; in the two-parameter case, a large β implied a low asymptote[7] or a rapid approach to this asymptote,[8] neither of which was a desired property; in the present model, however, it is possible to have a large value of β and also a reasonable asymptote in addition to a slow rate of absorption. The final point to note in Figure 6.1 is that the theoretical curve has not yet approached an asymptote. This suggests that the rate of absorption given by these parameter values is close to that in the data; moreover, the theoretical curve appears to underestimate consistently the observed. If the rate of absorption is adequate to describe the rate of becoming consistent drawn from the data, the fact that the present set of parameters underestimates the observed curve suggests that we desire to displace the theoretical mean curve so that it is about .05 greater than at present at each point. But such displacement would mean that the asymptote is greater than $\frac{2}{3}$. As we are not able to estimate the asymptotic probability of a correct response accurately, the asymptote may well be greater than $\frac{2}{3}$. After examining the present set of parameters, we shall consider the consequences of assuming an asymptote greater than $\frac{2}{3}$.

We have raised the issue of how well the theoretical rate of absorption compares with the observed. As we did with the one-parameter model, let us compare the predicted percentage absorbed on trials 10, 20, and 30 with the percentage of subjects showing consistent sequences from those trials onward. Table 6.3 presents this comparison. It should be remembered that the percentage of subjects showing consistent sequences is an estimate of the maximum percentage absorbed because not all subjects who are consistent are necessarily in S_1 or S_4. The table confirms the impression given by Figure 6.1 that the theoretical is close to the observed rate of absorption. Hence, this property of the model is adequate.

Table 6.3. Predicted and Observed Proportion of Subjects Absorbed on Trial n

(Moderate condition; three-parameter model: $\alpha = .05$, $\beta = .117$, $\epsilon = .032$)

From Trial Number	Actual Subjects Showing Consistent Sequences		Predicted Percentage Absorbed
	Number	Per Cent	
10	13	39.4	36.4
20	21	63.6	58.1
30	25	75.8	72.3

The next property of the model to examine is the distribution of errors. To obtain the theoretical distribution requires Monte Carlo computations. Once again, 10 samples of 33 subjects each were run using three-digit random numbers from the Rand Table (Rand, 1955) with the following transition rules:

1. If the subject is in S_2:
    a. If $950 \leq$ Random Number ≤ 999, the subject moves to S_1.
    b. If $117 \leq$ Random Number ≤ 949, the subject stays in S_2.
    c. If $000 \leq$ Random Number ≤ 116, the subject moves to S_3.
2. If the subject is in S_3:
    a. If $883 \leq$ Random Number ≤ 999, the subject moves to S_2.
    b. If $032 \leq$ Random Number ≤ 882, the subject stays in S_3.
    c. If $000 \leq$ Random Number ≤ 031, the subject moves to S_4.

In other respects, the procedure was the same as for the one-parameter model.[9] Table 6.4 presents the frequency and percentage distribution of errors grouped into class intervals for both Monte Carlo and actual subjects. Again the theoretical values are mean frequencies. The table shows that the shape of the Monte Carlo distribution is a good approximation to the observed distribution of errors. The largest discrepancy occurs in the class 0-4 errors. Part of this discrepancy can be accounted for by the fact that the Monte Carlo subjects tend to make more errors in general than did the actual subjects. This result was anticipated because the single Monte Carlo sample

Table 6.4. Comparison of the Distribution of Errors from the Monte Carlo Subjects with that Observed in the Moderate Condition

(Three-parameter model: $\alpha = .05$, $\beta = .117$, $\epsilon = .032$)

Number of Errors	Monte Carlo Subjects		Actual Subjects	
	Mean Frequency	Mean Per Cent	Frequency	Per Cent
0-4	12.5	37.9	16.0	48.5
5-9	3.4	11.2	2.0	6.1
10-14	3.1	9.4	3.0	9.1
15-19	3.8	11.5	4.0	12.1
20-24	2.8	8.5	2.0	6.1
25-29	2.3	7.0	1.0	3.0
30+	5.1	15.4	5.0	15.2
$\overline{X}$ =	12.8		10.8	
s^2 =	149.63		143.38	

that was used to choose α differed from the observed mean numbers of errors; hence, a more careful selection of α would reduce this discrepancy. Finally, the table shows that the theoretical and observed variances of the distribution match very closely.

Because our estimate of the population mean of the Monte Carlo subjects does differ from the observed mean, it would be profitable to examine how the mean of the distribution of errors varies from one Monte Carlo sample to another. By looking at the results of the 10 Monte Carlo samples, we can answer the question of whether or not the observed mean can be considered as drawn from this population. Table 6.5 presents the mean of each of the 10 samples. In addition, this table shows, for each sample, the variance and the statistic $\Pi_0 M$, the number of people who make precisely zero errors. For convenience, the observed values of each of these statistics are shown in parentheses at the bottom of the table. It is clear from this table that each of the observed statistics can be regarded as drawn from this population. One should, however, note that each of these sample statistics has considerable variability, for this fact bears on the problem of finding the optimum set of parameters. For example, the observed value of $\Pi_0 M$ plays a crucial role in determining the ratio of β to α; the Monte Carlo results in Table

Table 6.5. Sample Values of $\overline{X}$, s^2, and $\Pi_0 M$ from 10 Monte Carlo Samples

(Moderate condition; three-parameter model: $\alpha = .05$, $\beta = .117$, $\epsilon = .032$)

Sample	$\overline{X}$	s^2	$\Pi_0 M$
1	13.5	156.3	10
2	13.9	157.8	8
3	10.9	123.4	11
4	8.3	113.5	14
5	12.6	166.7	11
6	17.1	151.9	4
7	13.6	173.4	8
8	13.8	165.5	9
9	10.1	131.9	12
10	14.4	141.6	8
(Actual Subjects)	(10.8)	(143.38)	(10)

6.5 suggest that we have some flexibility in choosing a quantity for substitution in Equation 6.7.[10]

The properties where the most serious discrepancies between the model and the data occurred for the one-parameter model were the mean and the variance of the distribution of alternations. Does the three-parameter model adequately describe these statistics? We are in a position to compute the theoretical values of these statistics independent of the particular values of the parameters, provided that the ratios of the parameters are fixed. This computation will indicate whether we shall find a best set of parameters by further trial and error, maintaining the ratios derived from the estimates of Π_0 and $p(\infty)$, or whether we should revise the estimates of these statistics.

To compute the theoretical mean of the distribution of alternations, we first substitute Equations 6.25 in Equation 6.13 and solve for Z:

$$Z = \frac{\left(\frac{7}{3}\alpha\right)^2}{\left(\alpha + \frac{7}{3}\alpha\right)\left(\frac{7}{3}\alpha + \frac{7}{11}\alpha\right)} = \frac{11}{20} = .55 \qquad (6.27)$$

When we use this result in Equation 6.20, the mean number of alternations becomes

$$E(A) = \frac{2\alpha}{\frac{10}{3}\alpha}\,\frac{.55}{(.45)^2} + \frac{\left(\frac{7}{3}\alpha\right)\left(\frac{7}{11}\alpha\right)}{\left(\frac{10}{3}\alpha\right)\left(\frac{98}{33}\alpha\right)}\,\frac{1.55}{(.45)^2} = 2.77 \qquad (6.28)$$

The variance of the number of alternations is obtained from Equation 6.24, again by substituting the value of Z obtained in Equation 6.27. The result is

$$\sigma^2(A) = 11.13 \qquad (6.29)$$

Table 6.6 compares these computed results with those obtained in the Moderate condition.

Table 6.6. Theoretical and Observed Means and Variances of the Number of Alternations

(Moderate condition; three-parameter model: $\alpha = .05$, $\beta = .117$, $\epsilon = .032$)

	Theoretical	Observed
Mean Number of Alternations	2.77	6.79
Variance of the Number of Alternations	11.13	53.42

This table compares theoretical values based on an experiment with an infinite number of trials with the values observed in an experiment of 36 critical trials. Although most of the alternations occur in the earlier part of the experiment before a substantial number of subjects is absorbed, some alternation will occur after trial 36. Therefore, the discrepancy shown in the table is actually larger than the numbers indicate. Despite the fact that this model represents an improvement over the predictions of the one-parameter model (shown in Table 5.4), the discrepancy must be considered a serious one. At this point, we could examine theoretical statistics more comparable with the actual data by using the Monte Carlo samples, but there is little point in further investigation because the problem is already clear. Briefly stated, it is as follows: While it would be possible to improve the fit of the theoretical to the observed mean curve by finding the best set of parameters, given that $\beta = \frac{7}{3}\alpha$ and $\epsilon = \frac{7}{11}\alpha$, maintaining these ratios would not improve the fit of the model with regard to statistics related to alternations.

The ratios of β to α and ϵ to α depend on the estimated values of Π_0 and $p(\infty)$; the fact that Π_0 has considerable binomial variance and the fact that we are unsure of the value of $p(\infty)$ offer a possible solution to the problem that involves assuming values other than $\frac{3}{10}$ and $\frac{2}{3}$ for these statistics. There is, however, another alternative. We can use expressions other than those for Π_0 and $p(\infty)$ to estimate the ratios of the parameters. Unfortunately most of the possible combinations of the equations that we have derived are not readily solved; using the expression for $E(A)$ instead of $p(\infty)$ together with Π_0, however, does yield a solution. From Table 6.6, the mean number of alternations is 6.79, and again we have $\beta = \frac{7}{3}\alpha$. If we substitute these in Equation 6.21, the explicit expression for $E(A)$ gives

$$6.79 = \frac{\frac{7}{3}\alpha}{\alpha} + \frac{\frac{49}{9}\alpha^2}{\alpha}\left(\frac{\alpha - \epsilon}{\frac{7}{3}\alpha^2 + \alpha\epsilon + \frac{7}{3}\alpha\epsilon}\right)$$

Solving this equation for ϵ, we obtain

$$\epsilon = -.25\,\alpha$$

But this solution violates the restriction that all parameters must be between 0 and 1. Hence, this result further supports the view that we cannot fit the mean number of alternations without altering the ratios of the parameters. Before pursuing this problem further, let us examine the fit of the model to data from the Extreme condition. Such an investigation may provide some indication of what assumptions are necessary to improve the fit of the model to the Moderate condition.

6.4 Analysis of the Extreme Condition

The one- and two-parameter models were not tested with data from the Extreme condition because these models predicted $p(n)$ as a monotonic function of n. As we noted earlier in this chapter, this restriction is not present in the three-parameter case. Thus it might be possible to fit this case of the conflict model to data from the Extreme condition.

To estimate the parameters in the case, we shall again use Equations 6.7 and 6.18, the expressions for Π_0 and $p(\infty)$, respectively; this step will yield β and ϵ in terms of α. Following this, the trial-and-error procedure outlined in our analysis of the Moderate condition will give the value of α that produces a mean number of errors close to that observed in the Extreme condition. It will be remembered from Chapter 2 that the Extreme condition included 27 subjects, of whom there were 6, or 22 per cent, who made no errors. To facilitate computation, we shall deal in round numbers and use .25 as an estimate of Π_0. This estimate gives the relation

$$\beta = 3\alpha \tag{6.30}$$

when substituted in Equation 6.7. To estimate the asymptotic probability of a correct response, we refer to Figure 2.1. The mean curve for the Extreme condition presented in this figure shows a clear-cut trend and is apparently still rising at the end of the experiment. On the basis of this fact, it seems wise to choose a value for $p(\infty)$ that is greater than any value of $p(n)$ actually observed.[11] Thus $p(\infty) \triangleq .9$ would be a reasonable estimate. Using this value in Equation 6.18, we obtain

$$\epsilon = .115\alpha \tag{6.31}$$

Several values of α were then assumed, and a Monte Carlo sample of 27 subjects was run for each value. The set of parameters that gave the closest approximation to the observed mean number of errors was

$$\left.\begin{aligned} \hat{\alpha} &= .13 \\ \hat{\beta} &= .39 \\ \hat{\epsilon} &= .015 \end{aligned}\right\} \tag{6.32}$$

Repeated substitution of the values of Equations 6.32 in the transition equations (Equations 6.3) yielded values for $p_j(n)$ for all n. From this, it was simple to obtain the theoretical mean curve. This curve is presented in Figure 6.3 together with the mean curve.

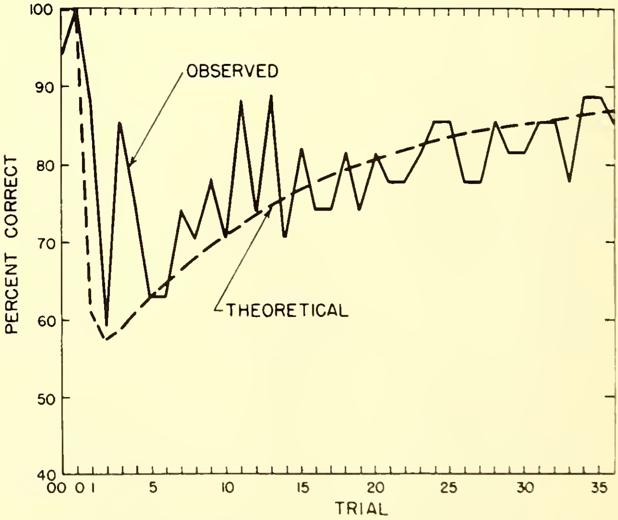


Figure 6.3. Comparison of the theoretical and observed mean curves. (Extreme condition; three-parameter model: $\alpha = .13$, $\beta = .39$, $\epsilon = .015$)

Although this set of parameters is not the optimum, the mean curve computed from these values is a reasonably good approximation to the observed. Figure 6.3 illustrates one discrepancy that should be mentioned. The theoretical curve is considerably off at trials 1 and 3, more, it appears, than can be accounted for by sampling variation. It is possible that these discrepancies could be eliminated with a better set of parameters. However, it might be that an additional assumption about the initial stage of the process would be necessary to improve the fit at the early trials.[12] Further experimentation is required for a thorough investigation of this particular problem; for example, the presence of neutral trials at the beginning of the sequence may be the factor producing the difference between early and later trials.

In computing the mean curve, we also compute $p_1(n)$ and $p_4(n)$. Using this information, we can compare the predicted percentage absorbed on trials 10, 20, and 30 with the number of subjects showing consistent sequences from those trials onward. This analysis will indicate whether the theoretical and observed curves are approaching the asymptote at about the same rate as Figure 6.3 suggests. The relevant data appear in Table 6.7.

Table 6.7. Predicted and Observed Proportion of Subjects Absorbed on Trial n

(Extreme condition; three-parameter model: $\alpha = .13$, $\beta = .39$, $\epsilon = .015$)

From Trial Number	Actual Subjects Showing Consistent Sequences		Predicted Percentage Absorbed
	Number	Per Cent	
10	13	48.1	54.5
20	18	66.7	77.6
30	24	88.9	89.0

The comparison suggests that there is some discrepancy, particularly at trial 20. Part of this discrepancy may be owing to measurement error, since three subjects who are not consistent from trial 20 on show exactly one inconsistent trial each. In any case the discrepancy is not serious.[13]

We consider next the distribution of errors. Again we are required to run Monte Carlo subjects to obtain the theoretical distribution. Thus 10 samples of 27 subjects were run for this purpose. In addition to the general procedure for the Monte Carlo computations discussed earlier,[14] the following transition rules were specific to the present case:

1. If the subject is in S_2:
   a. If 870 ≤ Random Number ≤ 999, the subject goes to S_1.
   b. If 390 ≤ Random Number ≤ 869, the subject stays in S_2.
   c. If 000 ≤ Random Number ≤ 389, the subject goes to S_3.
2. If the subject is in S_3:
   a. If 610 ≤ Random Number ≤ 999, the subject goes to S_2.
   b. If 015 ≤ Random Number ≤ 609, the subject stays in S_3.
   c. If 000 ≤ Random Number ≤ 014, the subject goes to S_4.

In Table 6.8 the results of the Monte Carlo computation are compared with the observed distribution of errors from the Extreme condition. (We have again grouped the data in class intervals; the theoretical cell entries are obtained by dividing the total frequency by 10.) The means and variances appear at the bottom of the table. In the present instance, the Monte Carlo mean is a better approximation to the observed mean than was the case for the Moderate condition. Furthermore, the variance of the Monte Carlo distribution is sufficiently close to the observed. The most serious discrepancy in the table occurs in the interval 10-14; to determine how much of this difference is caused by sampling variation would require a considerably larger sample. That is, it is possible that the empty cells in the sample distribution are accurate estimates of zero probability in the population, in which case the model would contain systematic error. On the other hand, if the model is correct,

Table 6.8. Comparison of the Distribution of Errors from the Monte Carlo Subjects with That Observed in the Extreme Condition

(Three-parameter model: $\alpha = .13$, $\beta = .39$, $\epsilon = .015$)

Number of Errors	Monte Carlo Subjects		Actual Subjects	
	Mean Frequency	Mean Per Cent	Frequency	Per Cent
0-4	13.8	51.1	17	62.9
5-9	4.6	17.0	5	18.5
10-14	3.4	12.6	0	0.0
15-19	1.4	5.2	0	0.0
20-24	1.0	3.7	1	3.7
25-29	1.3	4.8	0	0.0
30+	1.5	5.6	4	14.8
$\overline{X}$ =	8.0		7.7	
s^2 =	95.98		115.55	

the variance of the true probabilities (using mean percentage as estimates of these true values) would allow empty cells to occur. Therefore, until there is evidence to the contrary, it is reasonable to ascribe these discrepancies to sampling error. Although sampling variation can account for the differences between the theoretical and observed distribution, the possibility of systematic error was raised because it might be desirable to alter the parameter values slightly, a modification that would systematically change the theoretical distribution. Such a change might reduce the discrepancy between the theoretical and observed. For the present, however, we can regard the fit of theoretical to observed as satisfactory.

The sampling variability of the mean and variance of the distribution of errors and of $\Pi_0 M$ (the number of subjects with zero errors) are also of interest in connection with the model for the Extreme condition. Table 6.9 presents the sample values of these statistics for each of the 10 Monte Carlo samples. The observed values are clearly within the range of sampling variation. Comparing this table with Table 6.5, which presents the same data for Monte Carlo samples based on $\alpha = .05$, $\beta = .117$, and $\epsilon = .032$, poses an interesting problem for extensive Monte Carlo investigation. In Table 6.5, the range of sample means is 8.8, and the range of sample variances is 59.9. The ranges in Table 6.9 are 6.7 and 100.9 for the mean and variance, respectively. These differences may be an artifact of the small number of samples on which these ranges are based. The dif-

Table 6.9. Sample Values of $\overline{X}$, s^2, and $\Pi_0 M$ from 10 Monte Carlo Samples

(Extreme condition; three-parameter model: $\alpha = .13$, $\beta = .39$, $\epsilon = .015$)

Sample	$\overline{X}$	s^2	$\Pi_0 M$
1	7.6	85.4	7
2	8.9	131.0	8
3	6.4	87.6	10
4	3.8	30.1	8
5	10.5	83.9	6
6	9.2	91.7	7
7	7.1	129.2	8
8	8.4	109.5	12
9	9.4	84.2	5
10	8.8	128.1	7
(Actual Subjects)	(7.7)	(115.5)	(6)

ference between the ranges of the sample variances, however, seems to have some basis when we recognize the effect of increasing the parameter values. In the earlier model, $\alpha = .05$, $\beta = .117$, and $\epsilon = .032$, there is a higher probability of remaining in S_2 or S_3 for a long sequence of trials than in the present case, $\alpha = .13$, $\beta = .39$, $\epsilon = .015$. The higher probability of consistency would tend to stabilize the variance; on the other hand, the higher probability of movement for the larger parameter values tends to produce larger changes in the distribution of errors and hence larger variability of the variance from sample to sample. The full implications of this difference, if indeed it is a difference, are not yet understood. A fuller investigation of the parameter space of the model with a greater number of separate samples should shed light on this problem.

Although the questions are unsolved, the preceding paragraph has important practical implications. If the supposition is true, it would mean that in those cases where the larger parameter values apply, we should need larger samples to obtain more accurate estimates of the variance. That is, by reducing variation due to sample size, we should have to compensate for variation due to the model.

Because we have previously had difficulty fitting the distribution of alternations, it is important to examine these properties for the Extreme condition. We use Equations 6.30 and 6.31 to solve Equation 6.13 for Z:

$$Z = \frac{9\alpha^2}{(4\alpha)(3.115\alpha)} = .72 \qquad (6.33)$$

Then using Equations 6.30, 6.31, and 6.33 in 6.20, we find

$$E(A) = \frac{2\alpha}{4\alpha}\,\frac{.72}{(.28)^2} + \frac{.345\alpha^2}{(4\alpha)(3.115\alpha)}\,\frac{1.72}{(.28)^2} = 5.3 \qquad (6.34)$$

We then obtain the variance from Equation 6.24

$$\sigma^2(A) = 36.8 \qquad (6.35)$$

It will be remembered that these theoretical values are for an experiment containing an infinite number of trials. How they compare with the mean and variance of the number of alternations observed in the 36 critical trials of the Extreme condition is shown in Table 6.10. In contrast to the findings for the Moderate condition, the table shows that the theoretical values are greater than the observed, as is desired. This test of the model is then in the right direction; hence, we can investigate this area further.

Table 6.10. Theoretical and Observed Means and Variances of the Number of Alternations

(Extreme condition; three-parameter model: $\alpha = .13$, $\beta = .39$, $\epsilon = .015$)

	Theoretical	Observed
Mean Number of Alternations	5.3	4.8
Variance of the Number of Alternations	36.8	19.8

Table 6.11. Distribution of Alternations from 1 Monte Carlo Sample Compared with Data from the Extreme Condition

(Three-parameter model: $\alpha = .13$, $\beta = .39$, $\epsilon = .015$)

Number of Alternations	Monte Carlo Subjects Frequency	Actual Subjects Frequency
0	7	6
1	2	1
2	2	4
3	3	2
4	1	3
5	1	0
6	2	3
7	0	1
8	2	1
9	2	2
10	1	0
11	1	1
12	1	2
13	1	0
14	0	0
15	0	0
16	0	1
17	1	0
$\overline{X}$	4.9	4.8
s^2	23.4	19.8

To compare the distribution of alternations arising from the model in a 36-trial period with that actually observed, we have tabulated this distribution for the first of the 10 Monte Carlo samples. Table 6.11 presents the distribution of alternations for the first Monte Carlo sample and for the Extreme condition. The mean and variance of each distribution appear at the bottom of the table. These statistics are directly comparable because they are based on the same number of trials. There is remarkably close agreement between the Monte Carlo and the actual data with respect to the mean and variance of the number of alternations. In addition, the Monte Carlo closely approximates the empirical distributions at each interval.

There is one further property to investigate. Related to alternations are runs. Whereas an alternation is a change from one response to another, a run is a consistent subsequence of responses, and the number of runs is always one more than the number of alternations. Thus the analysis with respect to alternations yields the same information that would come from an analysis of the distribution of runs. However, runs vary in length, and run lengths can give additional information about the fit of the model to the data. Of course, since run lengths are not independent of the number of runs or the number of alternations, an error with respect to the number of runs would produce errors with regard to run lengths; for this reason, we did not consider run lengths in testing the model for the Moderate condition. On the other hand, if the model correctly predicts the number of runs, this does not mean that it will correctly predict the distribution of run lengths. For example, a subject who has 3 runs could have any combination of run lengths that added to 36. Hence, it is possible that the model could be systematically in error with respect to the distribution of run lengths.

It is important to consider run lengths because this property reveals much about the structure of the process. For example, subjects whose consistent subsequences were relatively equal in length would perhaps differ from those who had several very short and a few very long runs. Since the model is based on a process of absorption, and since there is initially a high probability of alternation, we should expect the model to describe an experiment containing a majority of the second type of subject. The frequency distribution of run lengths for a sample should then be skewed with the highest frequencies at runs of short lengths. Although it is the nature of the case that the greater the number of runs, the shorter their average length and the fewer runs, the longer they are (because the sum of the run lengths is 36M for the sample), the shape of the distribution is not determined by fixing the number of runs. The mean length of run is, of course, fixed once the number of runs is specified ($\overline{X}$ = 36M divided by the number of runs). Therefore, in comparing the model with the data, we are interested in the shape of the distribution of run lengths.

Run lengths were tabulated for each of the 10 Monte Carlo samples, and a mean frequency distribution is presented in Table 6.12 together with the actual distribution from the Extreme con-

Table 6.12. Distribution of Run Lengths from 10 Monte Carlo Samples Compared with Data from Extreme Condition

(Three-parameter model: $\alpha = .13$, $\beta = .39$, $\epsilon = .015$)

Run Length	Monte Carlo Mean Frequency	Actual Frequency
1	57.6	70
2	28.3	29
3	16.4	12
4	7.2	1
5	4.3	5
6-10	6.2	11
11-15	1.5	5
16-20	3.3	6
21-25	2.7	4
26-30	3.0	3
31+	13.4	10

dition. The data in this table were grouped into class intervals because of the small frequencies of longer run lengths. It should be noted that the intervals are not of equal size. As the table demonstrates, the two distributions have quite similar shapes, and there is also a close correspondence in cell frequencies. Both the Monte Carlo and the empirical distributions agree with the expectation; that is, they are highly skewed, with runs of length 1 and length 2 having the highest frequencies. We can conclude that the model is quite adequate in describing this property of the data.

For the Extreme condition, then, we have examined the model's predictions for the mean curve, the distribution of errors, the distribution of alternations, and the distribution of run lengths. With respect to each property, the theoretical closely approximated the observed statistics. Those discrepancies which were found in the mean curve and the distribution of errors could be eliminated by slight adjustment of the parameters. Since these parameter values were estimated using a trial-and-error procedure, the fact that some changes are necessary is nor surprising. It is clear, however, that the values $\alpha = .13$, $\beta = .39$, and $\epsilon = .015$ are sufficiently close to the optimum values to provide a clear indication of the adequacy of the model. The acceptance or rejection of a model depends on what alternative models are considered; we have examined these data from the point of view of the baseline models in Chapter 3 and the one- and two-parameter cases of the "conflict" model,[15] and we conclude that the three-parameter "conflict" model affords the most adequate description of the experimental data for the Extreme condition.

One discrepancy between the model and the data for the Extreme condition which would probably remain, even with an optimum set of parameters, is the fact that the observed proportion of correct responses shows marked fluctuation on the early experimental trials, while the theoretical p(n) is relatively stable. This suggests that an additional assumption about the early stages of the process is necessary; what assumption would be desirable is a question for future research. One object of additional experimentation on this problem should be to determine the effect of prefacing the sequence of critical trials with a pair of neutral trials.

6.5 Reconsideration of the Moderate Condition

The analysis of the Extreme condition brought to light some features of the three-parameter model that bear on fitting this model to the Moderate condition. In the first place, the expected number of alternations increases as Π_0 decreases; this is not surprising because, by definition, when there are fewer complete sequences of correct responses, there are more alternations. What is of interest, however, is the amount of change in the mean number of alternations for a slight change in Π_0. For example, with an asymptote of $\frac{2}{3}$, the mean number of alternations varies with Π_0 as follows:

$\Pi_0 = .25, \quad \bar{X} = 3.67$

$\Pi_0 = .30, \quad \bar{X} = 2.77$

$\Pi_0 = .33, \quad \bar{X} = 2.33$

The second feature to note is that the number of alternations increases as the asymptotic probability of a correct response increases. This was not true in the case of the one- and the two-parameter models; for those cases, a higher value of the asymptote meant that the process ended sooner, and therefore, the sequences contained fewer alternations. For this model, though, a higher asymptote can mean a longer process and thus more alternations; the computations for the Extreme case illustrate the point because the theoretical mean curve for this condition descends to a minimum and then begins rising toward the asymptote. These facts suggest that we can improve the fit of the model to the Moderate condition by increasing the estimate of the asymptote and decreasing the estimate of Π_0.

Several trial computations following the suggestions of the previous paragraph were conducted with disappointing but enlightening results. It is indeed possible to improve the degree to which the computed value of the mean number of alternations approximates the observed value. But the computed value is based on an experiment containing an infinite number of trials.

When a Monte Carlo sample of 36 trials is compared with the data, the discrepancy remains large. Why is this so enlightening? It appears that in addition to being larger relative to α, β must be considerably larger than the estimate .117 used in analyzing the Moderate condition. For example, in the Extreme condition, a β equal to .39 produced a Monte Carlo mean number of alternations of 4.9 where the observed value was 4.8. Therefore, in order to approach the mean of 6.8 observed in the Moderate condition, some value of β larger than .117 is desired. But a β of .39 for the Moderate condition would yield a theoretical mean curve that would bear little resemblance to the curve actually observed. This theoretical curve would reach a minimum too early and rise toward the asymptote too rapidly. Other values of β produce similar difficulties; hence, we conclude that it is not possible to find a set of parameters such that the three-parameter model would adequately describe alternations and at the same time fit other properties of the data from the Moderate condition.

It is possible to increase the number of alternations without drastically altering the mean curve, the rate of absorption, and other properties that we have examined. To do this requires that we abandon the three-parameter model. The restriction in the three-parameter case is that the probability of moving from S_2 to S_3 is equal to the probability of moving from S_3 to S_2 or, by using the notation of the general model in Section 4.2, that

$$\beta = \gamma$$

The magnitude of β, however, must be restricted; otherwise the minimum value of p(n) would be too low. A large γ, on the other hand, would increase the number of alternations, and what effect it might have on the other properties could be compensated for by adjusting the values of α and ϵ. Hence, in Chapter 7 it is desirable to consider briefly the four-parameter model.

6.6 Summary

This chapter examined a particular three-parameter model in which the transitions from S_2 to S_3 and from S_3 to S_2 were equally likely. Section 6.2 derived expressions for $p(\infty)$, the mean and variance of the distribution of alternations, and several points of the distribution of errors. It was not possible, however, to find three independent equations to use in estimating the parameters. An approximate method was suggested, and some of the problems of estimation were discussed.

We then analyzed the Moderate condition in terms of this model. This model came closer to describing the data than the alternatives previously considered. The discrepancies, however, were sufficient to warrant further investigation. The major difference between theoretical and observed values again occurred with respect to the distribution of alternations. The final section of this chapter demonstrated that it was not possible to improve the fit of the model to this distribution and at the same time maintain the restriction $\beta = \gamma$.

For the Extreme condition, the three-parameter model closely described all the properties of the data examined. One discrepancy between the model and the data occurred on two early trials of the experimental sequence; this suggested that an additional assumption about the early stages of the process may be necessary. We speculated that this assumption might be related to the fact that the experimental sequence began with two neutral trials.

One intention of this chapter has been to illustrate the procedures used in estimating parameters of a model and in testing the model's predictions. In connection with this objective, this chapter discussed several of the difficulties involved as they were encountered.

NOTES

1. It will be noted that this is the same expression which resulted in the two-parameter model (Equation 5.41).
2. A trivial solution here is $\alpha = 0$, $\beta = 0$, and $\epsilon = 0$. The acceptable intervals for each of the parameters, since they are probabilities, are $0 \leq \alpha \leq 1$, $0 \leq \beta \leq 1$, and $0 \leq \epsilon \leq 1$.
3. The actually observed values are $\Pi_0 = .3$ and $\Pi_1 = .09$. The value $\Pi_1 = .06$ arises from assuming a subject who made one error actually made two, the second of which was unrecorded; and $\Pi_1 = .12$ arises from assuming a subject who actually made one error was recorded as having made two errors.
4. This is the technical issue of sufficiency which will not be discussed in detail here but which is considered in Cramer (1946).
5. An investigation of the parameter space, feasible only on a large-scale digital computer such as the IBM 704, would enable us to choose a set of parameters more closely approximating a best fit. See Chapter 7.
6. This restriction follows from the fact that β and ϵ are probabilities. The restriction is "less than" instead of "less than or equal to," because the latter would be a trivial case of the model.

7. Increasing β while holding α constant lowered the asymptotic probability of a correct response.

8. Increasing β while holding the ratio of β to α constant implied increasing α and thus increasing the rate of absorption or approach to the asymptote.

9. Chapter 5, pp. 73 ff.

10. We used the proportion of subjects making 0 errors to estimate Π_0; this proportion has $\sigma^2 = \frac{p(1 - p)}{N}$. For $p = .3$, the variance is .0064 for a sample of 33. The standard deviation is .08; hence, the 2σ range on this proportion is .14 to .46. Thus, from considering binomial variance, we should be aware of this flexibility in estimation; the Monte Carlo results merely dramatize this fact.

11. This same consideration did not apply in the Moderate condition because the mean curve showed no marked trend.

12. This view is supported by the fact that all subjects were correct on the first critical trial in the Moderate condition. In addition, Asch had told the present author that in much of his research the first critical trial is different from the succeeding trials. It is possible that this might account for some of the difficulties in fitting the Moderate condition.

13. We raise the question of measurement error to emphasize a skepticism concerning differences owing to one or two responses. As was pointed out earlier (Section 6.2), a single incorrect recording can have wide ramifications. In the present case, there is some possibility of measurement error as there is an erasure on the recording sheet for one of the subjects.

14. Chapter 5, pp. 72 ff.

15. The data from the Extreme condition were not considered in detail for the one- and two-parameter cases, since the initial property examined for these two models was the fact that p(n) was a monotonic function of n, a property clearly absent in the data.

Chapter 7

THE FOUR-PARAMETER MODEL

7.1 Introduction

This chapter examines the general case of the conflict model. The model was described in detail in Section 4.2, where the transition equations were given by Equations 4.6. Before discussing this case, it should be noted that the problems encountered in Chapter 6 are also present in the four-parameter case: We are unable to derive mathematically a sufficient number of properties of the model, and we can find only two of the four independent equations necessary to estimate the parameters. For the present, we must resort to a trial-and-error procedure that yields extremely gross estimates of the parameters, although we can obtain some idea of the properties of the model from them. The labor necessary to find an optimum set of parameter estimates is beyond the scope of the present work; the intention of the present analysis is rather to determine whether this model shows sufficient promise to warrant further investigation. The analysis in this chapter, then, should be regarded as an exploratory study rather than a definitive test of the four-parameter model.

7.2 The Four-Parameter Model

Since the mathematical analysis of this model is identical in method with that of Section 6.2, we shall omit the derivations and simply present the principal results. These are as follows:

$$\Pi_0 = \frac{\alpha}{\alpha + \beta} \tag{7.1}$$

$$\Pi_1 = \frac{\alpha\beta\gamma}{(\alpha + \beta)^2} \tag{7.2}$$

$$\Pi_2 = \frac{\alpha\beta\gamma(1 - \gamma - \epsilon)}{(\alpha + \beta)^2} + \frac{\alpha\beta^2\gamma^2}{(\alpha + \beta)^3} \tag{7.3}$$

$$\Pi_{35} = \frac{\beta\epsilon(1 - \alpha - \beta)}{\gamma + \epsilon} + \frac{\beta^2\gamma\epsilon}{(\gamma + \epsilon)^2} \tag{7.4}$$

$$\Pi_{36} = \frac{\beta\epsilon}{\gamma + \epsilon} \tag{7.5}$$

$$p(\infty) = \frac{1 + \frac{\gamma}{\epsilon}}{1 + \frac{\beta}{\alpha} + \frac{\gamma}{\epsilon}} \tag{7.6}$$

$$E(A) = \frac{\frac{\beta}{\alpha}\left(2\frac{\gamma}{\epsilon} + 1\right)}{1 + \frac{\beta}{\alpha} + \frac{\gamma}{\epsilon}} \tag{7.7}$$

These results do not provide us with the necessary independent equations for estimating the parameters.[1] Further analysis may uncover additional expressions for other quantities than those we have considered; for the present, however, we must turn to numerical procedures in order to obtain values for α, β, γ, and ϵ.

The procedure we used may be outlined as follows: Choose a set of values for α, β, γ, and ϵ. Using these numbers, compute a theoretical curve of the probability of a correct response by repeated substitution in the transition equations (Equations 4.6) and repeated solution of Equation 4.4; i.e., $p(n) = p_1(n) + p_2(n)$. Then compare this theoretical mean curve with the observed curve of the proportion of correct responses. Choose a new set of numerical values and repeat the computation and the comparison. When a large number of sets of values has been examined, select that set of parameter values which generates the theoretical curve most closely resembling the observed mean curve.

There are, however, three difficult problems with this simple numerical approach. (1) Any useful application involves examining a large number of parameter sets, for which the computational labor is horrendous. (2) There is as yet no systematic method for choosing trial sets of parameter values. (3) The phrase "most closely resembling" must be explicated; that is, appropriate criteria of "goodness of fit" must be developed.

The magnitude of the computation involved is indicated by the fact that it takes a clerk with a desk calculator approximately one hour to compute the theoretical curve for one set of parameter values in a 36-trial experiment. If we then require an index of the agreement between theoretical and observed, we further expand the labor. If all we wanted was the information for a single set of parameters, desk calculation would be practical even for an experiment with a much larger number of trials. Suppose, however, we wanted to try out two values for each of the parameters, we should have 16 separate sets of parameters for computation. And indeed we should have to try many more than two values for each parameter to obtain anything like reasonable estimates. Numerical estimation, then, is feasible only with the aid of high-speed computing equipment such as the IBM Type 704. Fortunately, the procedure we have outlined involves performing the same operations with different numberical values; that is, it is highly repetitive. Hence, programming the estimation procedure is straightforward.

While the computer enables us to examine a large number of parameter sets, we still require a systematic procedure for choosing those sets to investigate. One approach would be to define a parameter space for the model and systematically sample this space. For our purposes, the parameter space consists of all possible differing sets of parameters such that the minimum difference between a parameter in one set and that same parameter in a second set is .01. For example, two sets of parameter values belonging to the space, as we define it, are

1. $\alpha = .01, \quad \beta = .00, \quad \gamma = .00, \quad \epsilon = .00$
2. $\alpha = .02, \quad \beta = .00, \quad \gamma = .00, \quad \epsilon = .00$

Since these parameters are probabilities, their values are restricted to the range 0 to 1. Therefore, it is possible in principle to investigate this defined space completely. But this is a task that would require an inordinate amount of time even on the most potent computer[2] since, if the parameters were independent of one another, there would be more than 10^8 sets of parameters in the space. Fortunately, the model imposes restrictions on the parameters, viz.:

$$0 \leq \alpha + \beta \leq 1.0$$

$$0 \leq \gamma + \epsilon \leq 1.0$$

In addition, previous analysis suggests that it is reasonable to restrict our sampling to that area of the space where $\alpha < .5$ and $\epsilon < .5$. With these restrictions, and some concessions to expediency, we can devise a practicable sampling plan.

Sampling the space in stages significantly reduces the amount of computation. Our restrictions lead to the following inequalities which define a subspace:

$$0 \leq \alpha < .5$$

$$0 \leq \beta \leq 1 - \alpha$$

$$0 \leq \epsilon < .5$$

$$0 \leq \gamma \leq 1 - \epsilon$$

Suppose for the first stage we impose a coarse grid on this subspace and sample sets of parameters that differ in at least one value by .10 instead of .01. This would mean examining 1,600 sets in the first stage and would require approximately three hours of 704 time. From these 1,600 sets, we then would select a small number of sets yielding theoretical curves most closely resembling our observed curve. These sets provide the basis for the second stage of sampling with a finer grid; here we should explore other values in the neighborhood of the values selected using increments of .01. A staging procedure like the one described makes the computer problem manageable.

Two-stage sampling, however, does not guarantee that we shall find the best-fitting set of parameters, because we do not know what happens between the points of the grid used in the first stage. For example, if we sample with increments of .10, our sample would include sets with α = .10, .20, .30. Suppose the best-fitting set had an α of .15 but that sets with α = .30 gave better fits than either α = .10 or α = .20. We should conclude from the first stage that the second sampling should be in the neighborhood of α = .30, but this would lead us to miss the best-fitting set. Such an outcome is possible; whether or not it is likely to occur cannot be evaluated until we have more information about how the model behaves under changing parameter values.

Perhaps the most difficult problem is that of developing a criterion of goodness of fit. At present, it is out of the question to consider a statistical test of goodness of fit because our comparisons do not meet the requirement of independent observations necessary for most tests. Instead, we must rest with an index that allows us to make relative comparisons between sets of parameters.

Indices usually employed in this type of situation are not without difficulties. For example, an index like chi square weights each trial equally; similar discrepancies between observed and expected values increases the index by similar amounts, regardless of where in the sequence of trials these discrepances occur. Since we regard the process as tending toward stability, it is likely that this procedure gives undue weight to trials later in the sequence. An illustration of what may happen in searching for the set of parameters with the lowest chi square emphasizes the problem. Suppose we have two sets of parameters, A and B. Set A leads to a function p(n), which departs radically from the data for the first 10 trials and then converges to a small discrepancy for the remainder of the sequence. Set B, on the other hand, yields a function that is much closer to the observed values on the initial trials but has a slightly greater discrepancy than A for the remainder of the sequence. Arbitrarily extending the number of trials in the experiment increases the probability that we shall choose set A over set B, and this seems undesirable.

While we should prefer an index that differentially weights discrepancies depending upon where in the sequence they occur, it is a major problem to develop such an index. If, however, we keep in mind the ambiguities connected with chi square, we feel we can use this index for gross discriminations. But we should not take the index value too literally. The result should represent an improvement over the guessing procedure of Chapter 6.

The foregoing discussion has not included all of the problems connected with estimating the parameters by sampling the parameter space. The issues raised are intended to underscore one point: The procedure outlined is an expedient rather than an ideal solution to the estimation problem. The use of the computer to sample the parameter space is not, in this case, a substitute for further mathematical analysis but a stopgap measure until we can find an analytic solution. This is not to say that the computer investigation is not of value in its own right; suggestions for a solution to the estimation problem as well as for new experimentation could result from even the "brute-force" approach of this study. On the other hand, additional analysis, particularly of the criteria of "goodness of fit" could lead to a more efficient and less primitive use of the computer. What should be emphasized is the complementary nature of the two lines of investigation, mathematical and computer. With this in mind, we can proceed to the application of our procedure to the Moderate condition.

7.3 Analysis of the Moderate Condition

We have indicated that the objectives of this analysis are to determine whether or not the model shows sufficient promise to warrant further application to this type of experiment. Such determination, of course, depends ultimately upon what alternative models are available. The limited analyses here, however, can tell us if it would be more fruitful to modify some of the model's highly restrictive assumptions or if we can proceed to apply the model in its present form.

In the first stage of the computer investigation, we planned to sample a subspace bounded by the following inequalities:

$$.01 \leq \alpha \leq .41$$

$$.01 \leq \beta \leq .91$$

$$.03 \leq \gamma \leq .93$$

$$.03 \leq \epsilon \leq .43$$

The setting of limits for each parameter was somewhat arbitrary, but the boundaries do represent practical considerations. Thus, we chose the sample points so that the first stage would exclude the cases discussed in Chapters 5 and 6, i.e., the one-, two-, or three-parameter models. Furthermore, the upper bounds on α and ϵ, together with the way the computation was programmed, restricted the sampling to that part of the subspace where, for a given value of α or ϵ, there was at least one larger value of β or γ. In other words, the computer would not sample extensively from that area where $\alpha > \beta$ or $\epsilon > \gamma$. Finally, the computer program ruled out those values within the preceding boundaries which are outside the parameter space, that is, where $\alpha + \beta > 1$ or $\gamma + \epsilon > 1$. For example, when the computer deals with those parameters sets where $\epsilon = .43$, it will consider only 6 instead of 10 values of γ, namely .03, .13, .23, .43, and .53. These restrictions yield a sampling plan that is reasonable as well as practical.

The author programmed this procedure for the IBM Type 704 scientific computer.[3] It was estimated that the first stage would take approximately three hours of computer time. For scheduling reasons, we divided the problem into two parts: the first testing those sets with $\alpha = .01$ and .11, and the second the remaining sets. Part 1 used about 80 minutes of machine time and provided information which enabled us, in the interests of economy, to omit part 2 entirely.

In general, the first machine run indicated that sets with $\alpha = .01$ fit the data more closely than sets with $\alpha = .11$. Hence, it did not seem necessary to pursue increasing values of α. The five best-fitting sets from part 1 were used to choose values for the second stage of the sampling. These sets are

α	β	γ	ϵ	χ^2
.01	.11	.33	.03	24.2
.01	.11	.23	.03	30.3
.01	.21	.63	.03	26.5
.01	.21	.53	.03	28.8
.11	.21	.43	.13	27.0

These figures enable us to narrow the ranges of α, β, and ϵ but do not indicate a neighborhood for γ. In view of the wide range for γ, using increments of .01 as planned for the second stage of sampling seemed impractical; such a plan would require testing 40,000 sets. To reduce this number to manageable size necessitated some rather arbitrary choices: For α and ϵ, we chose three values closer to the lower end of the range; for β and γ, we used increments of .03 instead of .01. These decisions reduced the number to 540 (3 α's, 4 β's, 15 γ's, and 3 ϵ's), but they did not take advantage of other information available from the first computer run. Inspecting the computer output suggested that it would be desirable to test a smaller value of ϵ than had previously been considered. In addition, it appeared that high values of γ yielded good fits only for high values of β; since $\gamma = .43$ was the best fit for $\beta = .21$, the top β value for the second stage, we decided to omit the six highest values of γ, and to sample an additional value of β instead. The values sampled in stage 2 were

α	β	γ	ϵ
.03	.12	.21	.02
.04	.15	.24	.03
.05	.16	.27	.04
	.18	.30	.05
	.20	.33	
		.36	
		.39	
		.42	
		.45	

Table 7.1. Parameter Sets with Smallest Chi Square Values from the Second Computer Run

α	β	γ	ϵ	χ^2
.03	.12	.21	.02	22.56
.03	.12	.24	.03	21.03
.03	.12	.27	.04	20.52
.03	.12	.30	.05	20.68
.03	.15	.30	.03	23.03
.03	.15	.30	.04	23.58
.03	.15	.33	.04	21.83
.03	.15	.36	.04	23.15
.03	.15	.33	.05	23.81
.03	.15	.36	.05	21.69
.03	.15	.39	.05	22.16
.03	.16	.33	.04	23.71
.03	.16	.36	.04	22.56
.03	.16	.39	.04	24.05
.03	.16	.36	.05	24.06
.03	.16	.39	.05	22.26
.03	.16	.42	.05	22.74
.03	.18	.42	.04	24.12
.03	.18	.45	.05	23.45
.04	.15	.27	.04	23.35
.04	.15	.30	.04	23.50
.04	.15	.30	.05	22.37
.04	.15	.33	.05	22.17
.04	.16	.30	.04	23.92
.04	.16	.33	.05	22.85
.04	.16	.36	.05	23.07
.04	.18	.39	.05	24.10
.05	.16	.30	.05	23.87

All combinations of these values again meant testing 540 sets.

The second computer run produced 28 sets of parameters with smaller chi square values than the best set of the first stage. The improvement, however, was small; for the best set $\chi^2 = 20.5$. These are shown in Table 7.1. The difficulties with chi square as an index together with the small magnitude of the differences shown in the table preclude automatically choosing the set with $\chi^2 = 20.5$ for further investigation. In the three-parameter model we had found that (see Chapter 6) the greatest discrepancy between the model and the data occurred with respect to alternations that are in turn related to the ratio of γ to ϵ. To increase our chances of improvement over the three-parameter case, we decided to select for further investigation the set of values that maximized this ratio. The set

$$\alpha = .03,\ \beta = .12,\ \gamma = .21,\ \epsilon = .02$$

has the highest ratio and was chosen.

Figure 7.1 compares the theoretical and observed mean curves. Since the observed curve was used in the estimation, it is not

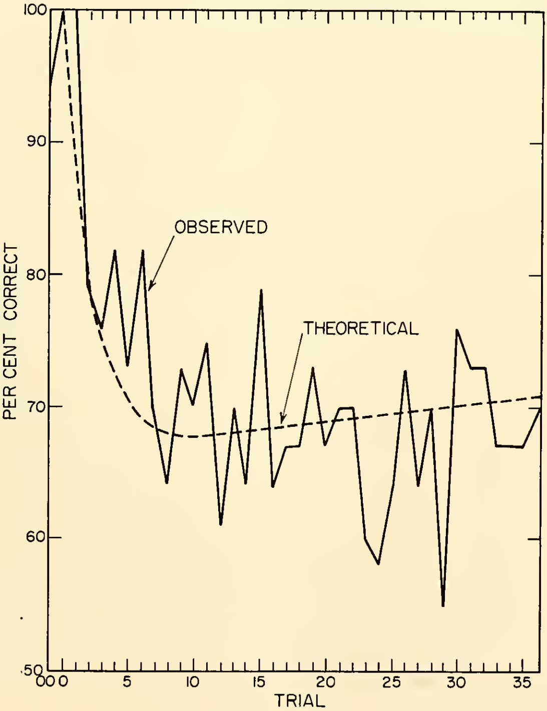


Figure 7.1. Comparison of the theoretical and observed mean curves. (Moderate condition; four-parameter model: $\alpha = .03$, $\beta = .12$, $\gamma = .21$, $\epsilon = .02$)

surprising that the fit is the best we have had thus far. If we compare Figure 7.1, for example, with Figure 6.2, we note that the theoretical curve does not consistently underestimate the observed, as was the case in Figure 6.2. In addition, the agreement between theoretical and observed curves for the early trials of the experiment represents a considerable improvement over the three-parameter model.

Since the estimated values of α and ϵ are smaller than those used in the three-parameter analysis, the process of absorption takes place at a slower rate in this model. This can easily be seen in Table 7.2, which compares the predicted percentage absorbed with the percentage of actual subjects showing consistent sequences. The differences between predicted and actual are larger than in the three-parameter case, but these differences do not indicate a failure of the model; it will be remembered that the percentage of subjects showing consistent sequences is an estimate of the maximum absorbed, including some who are in temporary states at trial n.

An estimate of the theoretical distribution of errors was calculated using 10 Monte Carlo samples.[4] The results are presented in Table 7.3. The Monte Carlo mean overestimates the observed, although less than it did in the three-parameter study. In addition, extreme scores are less frequent in the Monte Carlo results than in the actual data. These discrepancies do not appear serious in view of the fact that the variability among Monte Carlo samples is considerable. For example, 5 of the 10 Monte Carlo samples have means smaller than the observed mean. The result for extreme scores was anticipated because the theoretical quantity Π_0 equals .2, whereas the observed proportion of completely correct sequences equals .3.

The greatest discrepancy between theoretical and observed values in the one- and three-parameter models occurred when

Table 7.2. Predicted and Observed Proportion of Subjects Absorbed on Trial n

(Moderate condition; four-parameter model: $\alpha = .03$, $\beta = .12$, $\gamma = .21$, $\epsilon = .02$)

From Trial Number	Actual Subjects Showing Consistent Sequences		Predicted Percentage Absorbed
	Number	Per Cent	
10	13	39.4	24.2
20	21	63.6	42.0
30	25	75.8	55.6

Table 7.3. Comparison of the Distribution of Errors from the Monte Carlo Subjects with That Observed in the Moderate Condition

(Four-parameter model: $\alpha = .03$, $\beta = .12$, $\gamma = .21$, $\epsilon = .02$)

Number of Errors	Monte Carlo Subjects		Actual Subjects	
	Mean Frequency	Mean Per Cent	Frequency	Per Cent
0-4	10.6	32.2	16.0	48.5
5-9	5.8	17.5	2.0	6.1
10-14	5.1	15.4	3.0	9.1
15-19	3.7	11.2	4.0	12.1
20-24	2.9	8.8	2.0	6.1
25-29	1.7	5.2	1.0	3.0
30+	3.2	9.7	5.0	15.2
$\overline{X}$ =	11.8		10.8	
s^2 =	110.7		143.38	

we examined the distribution of alternations. Both of these models seriously underestimated the observed amount of switching. Does the four-parameter model then represent an improvement, or is the model as we formulate it incompatible with a set of data that includes both highly consistent and highly alternating sequences? In view of the provisional nature of the estimates of our four parameters, we cannot answer this question definitively. The results presented in Table 7.4, however, suggest that the model can generate alternating sequences. While the theoretical values in the table underestimate the observed, the discrepancies are considerably smaller than those of previous analyses. We noted earlier that the model predicted slightly fewer completely consistent sequences (zero errors) than were observed. It appears, then, that these parameter values improve the model's fit with respect to alternations at the expense of increasing

Table 7.4. Theoretical and Observed Means and Variances of the Number of Alternations

(Moderate condition; four-parameter model: $\alpha = .03$, $\beta = .12$, $\gamma = .21$, $\epsilon = .02$)

	Theoretical	Observed
Mean Number of Alternations	5.68	6.79
Variance of the Number of Alternations	40.37	53.42

discrepancies with respect to other properties. But since we do not regard these other discrepancies as serious in view of the variability of Monte Carlo samples, we feel encouraged by the results with regard to alternations.

One caution is necessary. The theoretical mean and variance in Table 7.4 are based on an experiment with an infinite number of trials; for a 36-trial experiment, these values will be smaller. The Monte Carlo data enable us to obtain theoretical values which are more directly comparable with our observations and which provide a more accurate reflection of the discrepancies between predicted and observed. Table 7.5 presents the distribution of alternations calculated from 10 Monte Carlo samples

Table 7.5. Distributions of Alternations from 10 Monte Carlo Samples Compared with Data from the Moderate Condition

(Four-parameter model: $\alpha = .03$, $\beta = .12$, $\gamma = .21$, $\epsilon = .02$)

Number of Alternations	Monte Carlo Subjects Mean Frequency	Actual Subjects Frequency
0	6.3	10
1	3.7	1
2	5.0	3
3	2.5	0
4	5.0	2
5	2.8	4
6	4.2	2
7	1.3	1
8	1.5	0
9	0.4	0
10	0.2	0
11	0.0	0
12	0.1	0
13	0.0	1
14	0.0	1
15	0.0	0
16	0.0	3
17	0.0	2
18	0.0	1
19	0.0	0
20	0.0	1
21	0.0	0
22	0.0	0
23	0.0	1
$\overline{X}$	3.3	6.8
s^2	7.0	53.42

together with the observed distribution. The Monte Carlo mean and variance shown at the bottom of the table are much further from the actual values than the computed mean and variance. However, when this table is compared with Table 5.4, we find that the four-parameter model fits alternations more closely than the one-parameter case.[5] Despite this improvement, the lack of fit is clearly still a problem. We shall return to this issue shortly.

Before deciding to modify the model or to seek new estimates of the parameters, we chose to collect additional experimental data. For this purpose, a new experiment was conducted, which we shall call Experiment 3, the aim of which was to replicate the Moderate condition. But Experiment 3 illustrates the interplay between the analysis of the model and the construction of experiments, for it was not an exact duplication of the Moderate condition. If the four-parameter model and the parameter estimates we have reported are taken seriously, nearly half the subjects are unabsorbed at trial 36 (see Table 7.2). If subjects were run through a longer sequence of experimental trials, we should have a better idea of equilibrium behavior. The design of Experiment 3 took advantage of the analysis of the Moderate condition by expanding the sequence to 80 trials. In addition, the new sample contained a larger number of subjects.

Experiment 3 took place at a different college from the one in which the Moderate condition was run. The population from which we drew subjects and the physical facilities of conducting the experiment differed from our previous study. The fact that our earlier results closely approximated those obtained by Asch with three quite different student populations suggested that population differences would be negligible. Nor had we any reason to believe that changing the physical setting of the experiment would markedly alter our results.

Fifty male undergraduates, all of whom were recruited through a student employment service and paid for their participation,[6] served as subjects. They were put through the same situation, except for length of the experiment, as Moderate condition subjects. That is, they judged the same stimuli, faced a unanimous and incorrect majority on all but the first two trials, and were asked to respond orally. Once again each of the 80 trials required the identical judgment; the standard and comparison lines were the same on every trial although the labels of the comparison lines varied in the same fashion as in the Moderate condition. As in the earlier study, to relieve monotony, we randomly placed the standard to the left or right of the comparison lines on each trial.

These data were collected to determine (1) whether or not the parameter estimates obtained from the computer investigation would fit a completely new set of observations, and (2) if some of the discrepancies between the model and the Moderate condition would disappear with more stable estimates of the summary statistics. It is important to emphasize that no data from Experiment 3 were used to obtain the parameter values; the numerical estimates from the Moderate condition were applied directly to the new observations. As far as Experiment 3 is concerned, the parameters are a priori, and no degrees of freedom are used up.

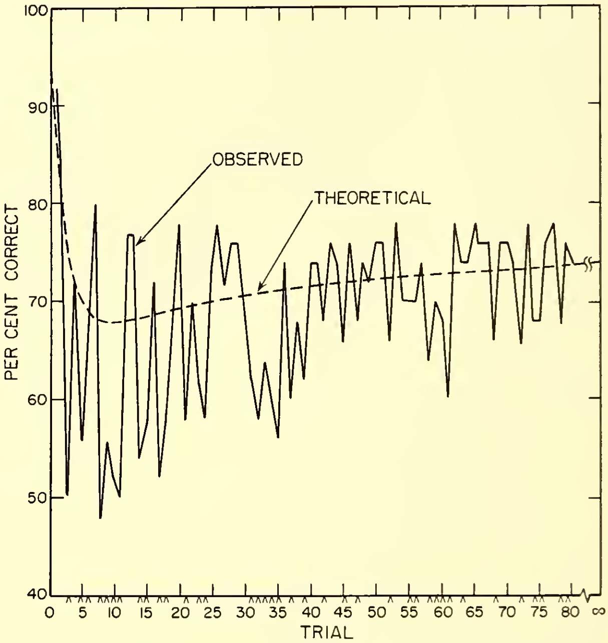


Figure 7.2. Comparison of the theoretical and observed mean curves for Experiment 3. (Four-parameter model with previously estimated parameters: $\alpha = .03$, $\beta = .12$, $\gamma = .21$, $\epsilon = .02$)

Figure 7.2 compares the theoretical mean curve with the curve observed in Experiment 3. On the whole, the fit is quite good; while there is considerable variability in the observed curve, it appears to stay near the theoretical. Second, the time trend in Experiment 3 is much more pronounced than in the Moderate condition; the 80-trial experiment indicates that the asymptotic probability of a correct response is considerably higher than we should estimate from observing a 36-trial experiment. This suggests that the reasoning in Chapters 5 and 6 which led us to reject those models that generate a monotone decreasing mean curve was essentially sound. Finally, although the variability of the observed curve decreases through time as the model predicts, on the initial trials this variability is larger than the Moderate condition. Since Experiment 3 involved a larger number of subjects than the Moderate condition, one would expect this variability to be smaller.

The large trial-to-trial variability of the mean curve for Experiment 3 points to a problem with respect to the distribution of alternations. Indeed, the mean number of alternations in Experiment 3 is 11.2, which is a greater discrepancy from the predicted than we observed in the Moderate condition. These results are convincing evidence that the discrepancy represents a systematic departure from the model rather than random fluctuation. The problem then becomes one of accounting for this systematic departure.

Reconsideration of Figure 7.2 provides an explanation for the unexpectedly large number of alternations; if we compare those trials marked with inverted v's (∧) with those not marked, we immediately see striking differences. Every large change in the proportion of correct responses occurs between a marked and an unmarked trial. The changes between two marked or two unmarked trials are relatively small. Furthermore, the observed values for all but one of the marked trials falls below the theoretical, while for nearly all of the unmarked trials, the observed is greater than the theoretical. This suggests that the predicted curve represents the average of two separate processes and that alternations occur when the experiment switches from evoking one of these processes to evoking the other. This interpretation will seem more apparent when the reader becomes aware that the marked trials have a significance independent of the observed data: Trials that are marked are those on which the standard line was presented to the left of the comparison lines, whereas unmarked trials presented the standard to the right of the comparison lines.

In Experiment 3, varying the side of the standard had a significant effect on the subject's behavior, but we have already noted in Chapter 2 that this device did not significantly affect responses in the Moderate condition. The differences between the outcome of these two experiments are more apparent than real. First of all, closer examination of Table B.1 in Appendix B shows that in the Moderate condition as well more errors were made when the standard was to the left than when it was to the right. While the results are not statistically significant, the trend in the Moderate condition is clearly the same as in Experiment 3.

The second point to be considered in evaluating the difference between the Moderate condition and Experiment 3 concerns the physical setting in which each experiment took place. The room available for running the Moderate condition dictated a seating arrangement that differed from Asch's original work. At the time, little importance was attached to this change, which involved seating the group in a semicircle with the naive subject one seat from the end. This arrangement placed the subject somewhat closer to the left stimulus display stand than to the right display. The chairs, however, were movable, so that by setting the circle off center it was possible to reduce, though not eliminate, the difference. This arrangement was carried over to Experiment 3, although the room employed presented other seating problems. Here we were assigned a lecture room with fixed seats and a fixed desk on which to set the display stands. To reproduce the semicircle, it was necessary to seat the naive subject closer to the left stand than in the Moderate condition. Furthermore, the fact that the chairs were fixed held constant the relative distances in Experiment 3, whereas these distances probably varied when subjects were free to move their chairs.

It should be noted that we were aware of potential problems arising from seating arrangement in Experiment 3. We equated the perceptual task of Experiment 3 with that of the Moderate condition by running four subjects without the presence of confederates. None of these subjects made a discrimination error, so that we felt the task was equivalent to the earlier study.

If the judgment task was not affected by the seating arrangement, why then should the two seating arrangements differentially affect yielding behavior? In both experiments, more yielding occurred when the standard was to the left, that is, closer to the subject. But the standard on the left means that the three comparison lines from which the choice is made are farther away. If we assume that moving the comparison lines away from the subject could be subjectively defined as increasing the ambiguity of the situation and that a subjective definition of ambiguity allows him to rationalize yielding to the group, we can account for the association of yielding with position of the standard. We can explain the different amounts of associations in the two experiments on the basis of a greater and more consistent distance from the comparison lines in Experiment 3 which leads to greater and more consistent ability to rationalize. The distance does not change the subject's judgment of the comparison lines but rather his ability to rationalize giving an incorrect response. When there is no need for such rationalization, as in the control group, the distances involved in these experiments will not produce differences in response. But given the need for rationalization, small distance differentials can produce the differences observed between the Moderate condition and Experiment 3.

Finally, in the original analysis of the effect of varying the standard in the Moderate condition, we focused exclusively on the association between frequency of yielding and position of the standard. Since this association was not significant, we ignored the position of the standard as a variable in subsequent analyses. Consideration of Figure 7.2 suggests that we examine the association between changes in the position of the standard and alternation of response. For this analysis, let us examine only those responses that change from trial n to trial n + 1. If our reasoning concerning subjective ambiguity is correct, then moving the standard from right on trial n to left on trial n + 1 should produce more changes from correct to incorrect than from incorrect to correct. Similarly, moving the standard in the opposite direction should have the reverse effect; that is, there should be more changes from incorrect to correct. The results presented in Table 7.6 are as we anticipated: When the standard moved from right to left, there were more than twice as many changes from correct to incorrect as from incorrect to correct; and when the standard moved from left to right, the pattern was exactly reversed.

Table 7.6. Association of Change in Response with Change in Position of the Standard

Response Changes	Standard Moves	
	From Right on Trial n to Left on Trial n + 1	From Left on Trial n to Right on Trial n + 1
From Correct on n to Incorrect on n + 1	52	25
From Incorrect on n to Correct on n + 1	25	53

The insights from Experiment 3 then bear directly on our analysis of the Moderate condition. We conclude that the discrepancy between the model and the Moderate condition is a systematic one which will not be eliminated with "better" estimates of the parameters. We attributed the discrepancy to a feature of the experiment which the model does not incorporate. The model considers all trials equivalent and assumes that the transition probabilities from trial n to n + 1 are constant, regardless of the nature of particular trials. Analysis of the Moderate condition shows, however, that there are several classes of transition events with which different probabilities should be associated. In other words, Table 7.6 is inconsistent with the assumption of constant transition probabilities; according to this assumption, transition probabilities should be the same whether the standard moves from right to left or from left to right.

At the beginning of this section, we noted that the purpose of these analyses was to determine whether we could apply the model in its present form or whether it would be more fruitful to modify some of the model's assumptions. It would seem from our results that modification of the constant transition assumption is in order. However, such a conclusion may be too literal an inference from our findings. While it is true that the present experiments depart from the model, our concern is with developing a general descriptive tool which would apply to new experimentation as well. Hence, in modifying the model, we must consider how any changes will affect the direction of future research. If a property of the experiment is to be incorporated into the model, it should be a property that is, in its own right, significant for future investigation or one that is a necessary concomitant of significant features of the experiment.

It would be possible to build into the model the variable transitions observed in our experiments. We could assume three sets of transition probabilities, one set associated with the event of moving the standard from right to left, one with the event of moving the standard from left to right, and one with the event of holding the position of the standard stationary. But by the criterion we have enunciated, this approach is not fruitful. Apart from the enormous difficulties of estimating 12 parameters, there is the serious question of associating basic elements of the model with incidental and accidental features of the experiment. Of course, increasing the number of parameters would improve the model's fit, but it would do so at the expense of giving equal weight to substantively crucial and substantively trivial features of the experiment. It is our contention that the position of the standard is a variable of minor substantive importance and should be eliminated from future experiments.[7] Insofar as the present experiments are concerned, we believe that we have isolated a source of error that confounds the core process which the model describes. If we recognize that any model is an idealization of its object phenomena and that our parameter estimates represent an averaging over nonessential confounding factors, we can regard the model as providing a sufficiently good description of the core process involved in the experiment. Despite the discrepancies we have encountered, we believe that it would be fruitful to continue to apply the model in its present form.

Throughout this monograph, we have been motivated by a desire to account for as much of the experimental variance as possible. The model does quite well by this criterion of its utility, particularly when compared with correlational attempts to account for conformity behavior. While it is clear that the model does not account for all the experimental variance, we have concluded that the sources of the unexplained variation are not of great theoretical significance. Rather than continuing the effort to increase the proportion of variance accounted for, it would be more strategic to indicate other criteria against which to evaluate the model's utility. Chapter 8 will offer examples of how the model may be utilized, but before turning to that discussion, there are some additional findings of our computer investigation which should be considered.

7.4 Computer Simulation of the Model

Up to this point, we have described the use of a high-speed computer in obtaining numerical estimates for the parameters. Now we want to emphasize another, and perhaps more important, aspect of applying the computer to this research. There are many properties of the model that cannot at present be uncovered by mathematical means, either because the appropriate mathematical techniques have not yet been developed or because those techniques which exist are too laborious to carry out. Here computers can play a crucial role; computer simulation allows us to trace out consequences of our conceptualization which cannot be derived formally. By simulating the model, we can gain new insights about its properties. Although we have not yet pursued simulation studies in any systematic fashion, some provisional results have provided leads for further research. We shall summarize these to illustrate what can be accomplished through simulation studies.

The procedure outlined earlier in this chapter represents a partial simulation of the model. We say "partial" because the computer program does not simulate individuals going through the experiment trial by trial; rather, it generates certain of the averages that would result if a population of subjects to whom a given set of parameters applied were put through the situation. Simulation of individuals generates the state for each individual on each trial through the use of random numbers. (This technique was described in Chapters 5 and 6.) The partial simulation, on the other hand, generates only the proportion of subjects in each state on each trial. Thus, for example, partial simulation tells us how the mean curve varies with changing parameter sets but cannot tell us anything about alternations, since alternations depend upon examining the change of state from trial n to trial n + 1 for each individual. Thus, statements about alternations must await the simulation of individuals. Similarly, from the partial simulation we can examine how the mean number of errors changes as we change the parameters, but we cannot say anything about the variance of the number of errors. The computer output from our partial simulation, however, offers a wealth of material dealing with the behavior of the mean curve and the mean number of errors.

Although the output is still being analyzed, we have uncovered two features of the model from our partial simulation. The first is a documentation of a point we have previously made, whereas the second represents a property of the model of which we were previously unaware.

We had noted that the mean number of errors may not be an appropriate statistic for describing this kind of experiment or for distinguishing between groups classified on some a priori basis and then subjected to the situation. We asserted that this statistic may not be sensitive enough to changes in the underlying process, that widely different processes could give rise to similar means. While this point is widely recognized, it can be documented only where there is a model that enables us to generate the process. The simulation study supports our contention but also requires us to qualify it. We selected those parameter sets yielding means between 10.7 and 10.9 and examined the ranges of our parameters in this selected sample. Of approximately 1,000 sets in the population for this preliminary analysis, 23 generated means within this very narrow range. Among the 23, the following ranges for each of the parameters were found:

α: .01 - .11

β: .15 - .71

γ: .27 - .93

ϵ: .02 - .13

With respect to α, β, and γ, nearly all the values used in the first and second stages of sampling the parameter space appear in these 23 sets. As we should expect, none of the high values of ϵ, that is, .23, .33, and .43, appear in the subsample, and this leads to our qualification. Over all the sets in the simulation, the mean number of errors varied from 3.0 to 29.0, a finding hardly consistent with our statement about insensitivity. But high means were associated with high ϵ values and low means with high α values. The means, then, are sensitive to those factors that radically alter α or ϵ. High values of these two parameters would describe processes terminating, for all practical purposes, within a very few trials. The means perhaps would be a sufficient statistic to describe a rapidly terminating process, particularly since rapid absorption would markedly reduce variability. For processes that stretch out in time, however, as indicated by low rates of absorption, the mean number of errors seems to vary within a narrow band at the same time that the paramenters β and γ are fluctuating over nearly the whole of their permissible range. It will be remembered that β and γ were interpreted as the probability of the individual changing his leanings toward and away from the group, respectively. Two situations with markedly different β's, for example, would, in our interpretation, be quite different even if the situation were identical in values of α, γ, and ϵ. (It seems reasonable to consider a situation where an individual has a high probability of changing his leanings at each trial to be sharply distinct from a situation where that individual has a low probability of changing his leanings on each trial.) If our interpretation of these parameters is correct, then the results indicate that widely differing processes give rise to similar means. Although the behavior of the mean number of errors needs to be studied in a simulation designed specifically for that purpose, we believe that present "anecdotal" results support our original contention, which we would now state as follows: Comparing means of different experimental treatments would be appropriate only in very special situations (which do not include the standard Asch-type procedures); for procedures such as the one we have used, testing mean differences between treatments, such as between our Moderate and Extreme conditions, risks overlooking properties that have greater discriminating power than the mean number of errors.

A more detailed and systematic analysis of the data presently at hand could perhaps indicate the conditions under which it is appropriate to examine the mean as a crucial statistic. Studies are now under way examining the behavior of these means as a function of the ratios of the parameters; specifically, we were interested in how the behavior of the mean number of errors varies with the ratio of β to α when the ratio of γ to ϵ is held constant. Furthermore, we intend to investigate the discriminating powers of other statistics usually computed in this kind of research as a function of the ratios of the parameters. These studies should provide us with information of value in designing future experiments as well as point to further simulation studies that should be done.

The most intriguing and significant outcome of this partial simulation study is that it has provided us with a new insight about the nature of the model itself. Since the model has absorbing states, it is clear that when all individuals have passed into an absorbing state, an equilibrium will have been reached, and no further changes in the process will occur. This might be termed "stationary" equilibrium. An unforeseen result of our simulation was to point to another type of equilibrium, in addition to the stationary equilibrium we have just described. Even a casual inspection of page after page of the computer output reveals a curious regularity: For several of our parameter sets in the second stage of sampling the parameter space, the mean curve stabilizes when a considerable proportion of individuals are still not absorbed. Although individuals are still changing their behavior, that is, are moving about between stages 2 and 3, the proportion correct remains the same from trial to trial. In other words, every individual who moves from state 2 to state 3 is balanced by an individual moving from state 3 to state 2. Hence, under certain conditions, we have what might be termed an "equal exchange" equilibrium (see Berger and Snell, 1957). Furthermore, the simulation indicates conditions for the occurrence of this equal exchange equilibrium, namely, that the probability of being absorbed in state 1, α, equals the probability of being absorbed in state 4, ϵ. The point in time at which this equilibrium is reached will vary depending upon the magnitudes of α and ϵ, but such a stabilization will always occur when these two parameters are equal.

We began this computer investigation with questions about the model and about fitting the model to a specific set of data. Even the provisional results we have thus far obtained lead us directly back to the laboratory. We can now ask what properties of our experimental situation will generate equal exchange equilibrium; what variables can we manipulate to produce a situation in which α is equal to ϵ? In view of our interpretation of the parameters α and ϵ, we can phrase this question in a substantively more pointed fashion: Under what conditions will an individual be as likely to reject his own judgment as he is to reject the group?

7.5 Summary

This chapter has focused on the fit of the four-parameter model to the Moderate condition. After presenting the principal formal results of this model, we described a computer approach to estimating its parameters. This approach, it was emphasized, represents a stopgap measure until further analytic procedures become available. Nevertheless, the estimates obtained from the computer provided the basis for a general conclusion about the model and for a new experiment.

Our conclusion is that though the four-parameter case is an improvement over previous cases, a discrepancy between the model and the data from the Moderate condition would persist even if a set of best-fitting estimates could be obtained. The outcome of a new experiment, Experiment 3, enables us to identify the source of the discrepancy between the model's predictions and the observed data. From an analysis of Experiment 3 and a reanalysis of the Moderate condition, it is clear that varying the position of the standard line creates a situation which violates the assumption of constant transition probabilities.

On the grounds that alternating the position of the standard is a substantively unimportant feature of the experiment, we argued that it would be more fruitful to modify the experimental procedure rather than the model. Recognizing that a model is an idealization, we concluded that the present model provides a sufficiently good description of the core process involved in these experiments.

Finally, we indicated how computer simulation can contribute to a deeper understanding of the model. Provisional results of a partial simulation indicated that the mean number of errors was quite sensitive to changes in absorption rates and not at all sensitive to changes in β and γ. The most important results of the simulation, however, was the discovery of an equal-exchange type of equilibrium. This discovery leads back to the laboratory with the question: Under what conditions is an individual as likely to reject his own judgment as he is to reject the group?

NOTES

1. See Chapter 6.

2. For example, the IBM 704 can investigate about 1,000 sets per hour.

3. We are indebted to the Computation Center at M. I. T., International Business Machines, and Standard Oil of California for their aid in providing access to computers and funds for the development and running of our program. We are particularly grateful to Dr. Roger Simons of IBM for his invaluable assistance.

4. The transition rules for these Monte Carlo samples are

   1. If the subject is in S_2:
      a. If $970 \leq$ Random Number ≤ 999, the subject moves to S_1.
      b. If $120 \leq$ Random Number ≤ 969, the subject stays in S_2.
      c. If $000 \leq$ Random Number ≤ 119, the subject moves to S_3.
   2. If the subject is in S_3:
      a. If $790 \leq$ Random Number ≤ 999, the subject moves to S_2.
      b. If $020 \leq$ Random Number ≤ 789, the subject stays in S_3.
      c. If $000 \leq$ Random Number ≤ 019, the subject moves to S_4.

5. See Chapter 5, p. 82.

6. Analysis of the Moderate and Extreme conditions, where

both paid and unpaid subjects were used, confirmed our belief that payment did not markedly affect the experimental results. See Cohen (1956).

7. A pilot study with 10 subjects has been conducted in which the position of the standard was held constant. Although the number of subjects is small, it does appear that this modification markedly reduced the average number of alternations.

Chapter 8

AN OVERVIEW OF THE CONFLICT MODEL

8.1 Introduction

The last three chapters have concentrated on formal aspects of our model: derivation of the properties of the respective models and evaluation of the fit of these models to experimental data. The copious detail of this examination may have obscured the role of model building in contributing to our understanding of the phenomenon of conformity. But our major concern is with the substantive utility of the conflict model. In this chapter, then, we shall emphasize the theoretical issues that have emerged in the course of our work. While much remains to be done before we shall be able to formulate an explanatory theory of conformity, we believe that the conflict model represents a significant first step toward our stated goal. In formulating this model, we have not restricted ourselves to the concrete and specific nature of a particular empirical study; neither have we presented a set of interpretive principles completely unconstrained by observations. The conflict model, as an organizing tool, incorporates a few key ideas about conformity and explicates the relations among these ideas. In so doing, it serves as a baseline from which to proceed to cumulate knowledge.

The task that remains is to highlight our key ideas and to illustrate how one proceeds from the baseline we have established. Section 8.2 reviews the development of the conflict model, pointing out the principal ideas and their organization. In Section 8.3, we illustrate the application of the model to the test of two hypotheses drawn from current theorizing and also present a generalization of the model. The final section briefly inventories some of the immediate next steps in extending the model.

8.2 The Development of the Conflict Model

We began this monograph with the observation that previous applications of the Asch situation were not, in other than a correlational sense, attempts to explain behavior in the experiment.[1] That is, these investigations were aimed at explaining a set of conformity scores on the basis of the correlation of these with scores on some extrinsic variable, or with indicating differences in mean conformity by some a priori criterion of classification. But, as we have noted several times, there are a number of assumptions implicit in the use of these indicators of conformity behavior. It is to Asch's credit that he recognized some of these assumptions and attempted to examine them.[2] In the first instance, our study has been a direct continuation of Asch's work; we have explicitly stated some major assumptions concerning this conformity phenomenon, examined the adequacy of these assumptions, and asked against what alternatives they should be tested. But our concern did not end with the step-by-step examination of single assumptions independent of one another. The goal of explanation required that we seek a set of interconnected assumptions from which we could derive a large number of properties of the phenomenon — not only the values of various behavioral indicators but also the interrelations of these indicators.

Our desire to construct a model that would explicitly tie together our assumptions forced us to abstract from Asch's original work. The number of factors operating in Asch's Experiment 1 would require a battery of assumptions about the factors themselves, and the interactions among these factors, that would be much too complex to handle. Of necessity, we asked: "What is the core of this phenomenon?" Asch himself provided the basis for our answer; the essential experimental conditions, according to him, are the introduction of an unambiguous stimulus, the presence of a unanimous majority, and the necessity to respond publicly, whereas the principal regularity occurring under these conditions is the striking consistency of individual behavior. Guided by his insights, we decided to modify the experiment, removing such features as variations in the stimuli and trial-to-trial switching between moderate and extreme responses by the confederates. These modifications took place before any specific model was formulated. And, as Chapter 2 indicated, our first encouraging result was that these changes in procedure did not alter dramatically the observations obtained.

Here we should digress momentarily to point out two important aspects of this experimental simplification. Insofar as modifying the experiment reduces the variability of response, it should make the Asch situation more efficient for hypothesis testing. If there is a difference in the average amount of conformity between two experimental conditions, then reducing the variability due to extraneous factors should make it easier to detect this difference. The second aspect of the simplification is that the modifications themselves raise significant questions. For example, the decision to separate moderate and extreme trials into the Moderate and Extreme conditions leads us to examine how a "pure" sequence would differ from Asch's mixed sequence. We pursued this particular analysis and shall present the results in the next section.

Initially, the efforts to construct a model were devoted strictly to describing the observed patterns of conforming and nonconforming responses. The baseline models of Chapter 3 represent the examination of a few, simple, alternative assumptions that could account for these patterns. The "constant probability model" and the "individual differences model" both focused on previous views of the phenomenon: Asch's finding of the absence of temporal trends and his emphasis on individual differences. By attaching a different probability of responding correctly to each individual and regarding each of these probabilities as constant over time,[3] we incorporated both individual differences and the already noted within-individual consistency. Discovery of the inadequacies of this model demonstrated that not only individual consistency but also within-individual variability had to be taken into account.

Although we began this research with a strong belief in the importance of the process involved in the experiment, we had no empirical support for this belief until we had completed the analysis reported in Chapter 3. As a result of Monte Carlo simulation of the "individual differences model," it became clear that examining the temporal aspects of the experiment could contribute to our understanding of the conformity phenomenon.

Bush's "insight model" and the "consistency of group pressure model" were attempts to posit processes that would generate considerable individual consistency and yet allow some individuals to fluctuate. In the first case, consistency resulted from "insight," whereas variability was attributed to essentially random responding prior to insight. While we did not reject this model for the Extreme condition, it clearly did not fit the Moderate. The "consistency of group pressure model," applied to Asch's original Experiment 1, emphasized fluctuation, linking variation in the subject's response to variation in the confederates' answers. Because of this emphasis, the model did not allow enough individual consistency and hence generated a theoretical distribution of correct responses differing markedly from the observed.

Up to this point, the models examined stayed very close to manifest events. By and large, the elements of these models are either formal translations of summary statements about the observations or rigorous linkages between two sets of overt events. Positing a set of probabilities attached to individuals is one translation of individual differences, while predicating changes in the individual's conformity on changes in the confederates' responses is an example of a linkage. In other words, these models were not far removed from the phenomenal world; there were direct operational coordinates for the elements of the model. In attempting to understand why none of these models adequately described our experiments, we had to invoke considerations that had no direct operational meaning and, in so doing, we began to view conformity from a more abstract level. This experience convinced us of the necessity for turning one's back on the data temporarily and theorizing about the phenomenon.

The conflict model is much further removed from the phenomenal world. Its elements do not have direct operational coordinates. It formalizes a process that is not observable at the present time but rather is hypothetical and inferred. We formulated an underlying conflict resolution process to account for the observed process and then constructed a model to describe this underlying process. Once the decision to leave the restricted focus of the observed response was made, the utility of a more abstract approach became clear.[4] We had much more information about the conformity phenomenon than was contained in the observed response patterns; for example, the interview protocols from our own and previous studies, while not systematic nor amenable to formal treatment, provided many insights. The more abstract conceptualization allowed us much greater freedom to make use of this information as well as to make use of the substantive interpretations contained in prior studies. Many of the distinctions in the conflict model can be traced directly to these sources. Although we were unable to find empirical counterparts for some of these distinctions, they served valuable heuristic purposes.[5]

The idea of conflict has been a recurrent theme in discussions of the Asch experiment, and it seemed a reasonable point of departure for our thinking. Previous work, however, employed the notion largely in an intuitive manner; hence, the concept of conflict remained undefined and unanalyzed. The effort to explicate some of the features of a process involving conflict contributed to the construction of the four-state model.

The first distinction made was between conflict and resolution. In the Asch situation, environmental pressures generate conflict in the individual, but does this conflict persist throughout the entire sequence of experimental trials? Interview protocols suggested that some subjects recognized "being in conflict" but that this subjective state terminated before the end of the experiment. There were, then, at least two subjective states that subjects implicitly recognized; we termed these states, "conflict" and "conflict resolved." The notion of movement in time from conflict to conflict resolution arose quite naturally, given our concern with temporal processes. By assuming the simplest form of conflict, that between two opposing forces, and arguing that behavior differs, depending upon which of these forces is paramount, we arrived at the four states of our model. In our view, these subjective states logically exhausted the possibilities at any point in time, and so were sufficient to characterize where the process stood when examined at a particular time point.[6]

The interplay between the conflict process as we had thus far delineated it and our knowledge of the experiment generated further ideas. These ideas may not be universal attributes of conflict processes, but certainly are attributes of a restricted class of conflict. One salient question was, "How could this conflict process account for the striking consistency of individual behavior occurring side by side with highly variable behavior?" If resolution were irreversible, then passage to a particular resolution state, say resolution on the side of conforming to the confederates, would guarantee consistent behavior from that time on. Therefore, we assumed that once the conflict is resolved, it stays resolved. Assuming movement back and forth between the two conflict states allowed for variability of response and at the same time was consonant with the view that vacillation is a frequent accompaniment of conflict. At this point in our thinking, we had allowed movement between states 2 and 3 and barred movement out of states 1 and 4. We then considered the question of how one moves into the resolution states. Here, looking at our modifications of the experimental procedure was helpful. The experiment involves a smoothly flowing sequence of trials with intertrial intervals that are approximately constant and with similar behavioral requirements at each trial. There are no abrupt shifts in the task, nor are there sudden changes in environmental pressures. It seemed then that there should not be abrupt resolutions of the conflict, and we regarded passage from a conflict state where the individual leaned toward maintaining his integrity directly to a resolution state on the side of conforming to the confederates as too abrupt. Thus we restricted the mode of resolution of the conflict: The conflict cannot be resolved by passing from a conflict state in which one pole predominates directly to a resolution state where the opposite pole is dominant.[7] Adding these "movement" properties provided the transition rules among the four states and so extended the conception of the underlying process.

Substantive reasoning did not lead us directly to the conflict model. That is, we were not, and are not now, able to deduce the Markov chain model from substantive assumptions about the process of conflict resolution alone. In fact, the properties described in the last few pages do not allow powerful enough deductions to make relating the conflict process to data worth while. Additional assumptions were required before we could make further deductions. Our intuitive notions of conflict did not provide, in themselves, guides for these additional assumptions. We then searched for those conceptual tools that would enable us to make a large number of deductions. We sought a mathematical system that incorporated the properties attributed to the conflict process.

A number of formal systems could incorporate our ideas of conflict, and any of these would impose more constraints on the nature of the process than we could justify by the level of substantive understanding of conflict. But only by imposing additional restrictions could we tie the underlying process to the observed conformity responses. And just as we cannot justify these constraints, neither do we have enough knowledge to reject them wholly.

Another brief digression is necessary. If the elements of the conflict process could be tied directly to observables, then our problem would have been less difficult, because the observed responses would impose additional constraints. For example, do we choose a system that requires constant transition probabilities among the states? If we could directly observe the state to state movement, we could test whether such an assumption were tenable. But the conflict process, in our view, is a hypothetical, underlying process that cannot have one-to-one links with observable conformity behavior. To be sure, the overt response process is a function of the underlying process but is not identical with it.[8] Restricting ourselves to the overt behavior might have made choice among models a simpler task but might also have precluded finding an adequate descriptive system.

Since we viewed the conflict process as probabilistic, one path of investigation was to attempt to describe it by means of a Markov chain. This required several additional assumptions: (1) The transitions between the states are probabilistic; (2) the initial distribution among the states is known or can be ascertained; (3) the state at a point in time depends only on the state at the immediately previous time and the transition probabilities; (4) the transition probabilities are constant through time. In principle, these were the only additional assumptions needed and, at the outset of the study, did not seem overly restrictive. Assumption 2 in its weak form, for example, implies that we regard the initial distribution $p_1(0)$, $p_2(0)$, $p_3(0)$, $p_4(0)$ as parameters to estimate from the data. By so doing, we should have added considerably to the labors of derivation, but the complications introduced would have been complications of algebra and not of the theory. In practice, however, labor-saving devices were a consideration. Furthermore, in tying a model to a set of observations, other practical issues arose; in any experiment, there is a relatively limited number of observations, and knowledge about this is quite incomplete. In other words, it was desirable to have as few parameters as possible; this end necessitated further specifying assumption 2 and adding an additional premise: (2') $p_2(0) = 1.0$, and (5) the process has the same transition probabilities for all individuals.[9]

The coordination of elements of the model to observables was relatively straightforward. Point in time was operationally related to the point of the subject's response on an experimental trial. Correct responses were tied to presence in states 1 or 2, while conforming responses were linked to presence in states 3 or 4. The only issue that came up concerned whether or not to identify the start of the process with the first observed trial. Had we done so, we could have relaxed assumption 2' and made use of the responses on trial 1 to help estimate the initial distribution.[10]

Our examination of the conflict model proceeded from the most restrictive case to more general cases. It was clear than even the highly simplified one-parameter case provided a better description of the Moderate condition than did any of the baseline models. Using the mean number of correct responses to estimate the parameter, we were able to deduce a theoretical curve that closely resembled the observed time trend. In addition, the theoretical variance of the distribution of conforming responses more closely approximated the observed than in any of our previous analyses. While this model grossly underestimated alternation, it nevertheless provided us with considerable descriptive power.

The three-parameter model showed further promise. It provided us with a close-fitting description of the Extreme condition and an improved fit for the Moderate. This was particularly encouraging in view of the necessity for "guesstimating" the value of one of the parameters. For the Extreme condition, the model generated theoretical values that did not depart markedly from the observed for every quantity examined, including alternations and runs.

A trial-and-error computer procedure yielded numerical values for the four-parameter case. While these values were very gross approximations, they were sufficient to provide new information concerning the relation of the model to the Moderate condition. While the theoretical quantities for the four-parameter case were closer to the observed statistics,it became clear that better estimates of the parameters would not reduce substantially the major discrepancy between model and observation. Our analysis, and the data from a new experiment, indicated that the discrepancy with respect to alternations was due to a feature of the experimental procedure. Varying the position of the standard line resulted in a set of observations that failed to meet the assumption of constant transition probabilities.[11] We concluded that this feature of the experiment was not sufficiently important to require us to modify assumption 4 of the model. In future experiments, the variable can be controlled; for the present, we regarded the model as providing a sufficiently good description of the core process involved in the experiment.

There are other aspects to the utility of the conflict model in addition to its descriptive power with respect to the particular experiments that have concerned us. The heuristic value of the model is at least as important. In discussing its basic assumptions, we have indicated several directions for extending the model.[12] Moreover, we have asserted that the source of the model's theoretical utility lies in the hypothetical nature of the process it formalizes. By viewing conforming behavior as a function of an underlying process of conflict resolution, we are led to consider other overt behaviors that also may be functions of this underlying process. Furthermore, our formulation suggests that we examine not only other overt behaviors in Asch-type experiments but also responses in experiments where the situation is manifestly quite different but where the elements of conflict are also present. Application of a model of this underlying process to apparently divergent phenomena could provide the basis for interrelating a number of response variables and thus contributing to the development of a propositional structure.

At present, however, it is premature for us to move on to the study of other response processes in different situations. There are still many features of the present experiments to be examined. Since we have no behavioral data other than the sequence of conforming and nonconforming responses, we can only speculate about additional behaviors that may be functions of the underlying conflict resolution process. It is equally pertinent, however, to look at the conflict process itself as a function of variable properties of the experimental situation. These properties may be termed boundary conditions and include such things as nature of the stimulus, characteristics of the confederates, characteristics of the sample of subjects, etc. Any single experiment usually takes place under fixed values of these boundary variables, but from experiment to experiment the boundary conditions may change. It there is a change in some boundary variable, we should also expect a change in some aspect of the underlying process. For example, the Moderate and Extreme conditions of our experiments represent two different values of a boundary variable, and we can ask if the models for these two conditions reflect such a difference. In our view, the parameters of the model will vary with variation in certain boundary variables, while the initial distribution among the states will change as a function of other boundary conditions.

The underlying process serves as an intervening link between two kinds of observables, what we have termed boundary conditions and response processes. The conflict model has the potential for interrelating a large number of factors, tying them together through their relationship to the underlying process. In the next section, we shall apply the model to research questions that illustrate both the heuristic value of the model and avenues of possible extension of our conceptualization.

8.3 Application of the Conflict Model

In the previous section, we suggested that one way to extend the conflict model involved viewing the parameters as functions certain boundary variables. It is not difficult to enumerate many boundary variables, but at present we have data relevant to only one, namely, the nature of the confederates' responses.[13] The decision to separate moderate and extreme responses of the confederates into two conditions of the experiment naturally raised the question of differences between the two conditions. In Chapter 2, the author noted his belief that the confederates exert a differing amount of social pressure on moderate trials from that exerted on extreme trials. On the basis of this interpretation, one would certainly expect the two conditions to differ. And indeed at several points in our analysis, we noted important differences between the Moderate and Extreme conditions.

Up to this point, however, we did not focus on the differences between the Moderate and Extreme conditions per se. We postponed consideration of these differences because we believe that the conflict model provides us with a better means of summarizing them than simply comparing two time trends or presenting two means. Furthermore, and of more importance, the conflict model allows us to tie the differences between Moderate and Extreme conditions to a current theoretical position to which the author feels his work is closely related.

Once we are able to summarize in a simple manner the variation between the Moderate and Extreme conditions, can we use the description to examine the question raised in Chapter 3, for example, how does a pure sequence of either moderate or extreme trials differ from a "mixed" sequence? In examining this issue, we shall use the model and parameter values for the Moderate and the Extreme conditions in an attempt to fit the observations from Asch's Experiment 1.

This section, then, has two purposes: (1) to indicate the model's utility in summarizing the effects of a specific boundary variable in a theoretically relevant way, and (2) to show how the model may be used to construct more complex experimental sequences. To accomplish the first, we shall "test" two hypotheses that could not be tested using statistics usually examined in this type of study. Our second aim requires us to formulate a generalization of the conflict model, which we shall call the "two-event conflict model."

Festinger (1950) and Schachter (1950) offer some hypotheses relevant to the present experiment. These concern how the discrepancy between the group (or confederates) and the individual affects the tendency of the individual to change his opinion and also how it affects the tendency of the individual to reject the other members of the group. For the present purposes, it is desirable to paraphrase their hypotheses; where they speak of "forces acting on the individual," this author will use "the probability of an individual acting (in a specified manner)." The hypotheses are the following:

1. With increasing discrepancy between the group and an individual, the probability of that individual changing his response in the direction of the group's response should increase.
2. With increasing discrepancy between the group and the individual, the probability of that individual rejecting the group should increase.

Now the distinction between the Moderate and the Extreme conditions of the present experiment is a difference in the degree of discrepancy between the group's response and what the subject perceives, at least initially, as correct. The discrepancy between the group of confederates and the subject is greater in the Extreme condition. Then should the mean number of conforming responses be greater or smaller in the Extreme condition? Hypothesis 1 says that the Extreme should be greater than the Moderate, but from Hypothesis 2 we can predict the reverse. Using these hypotheses jointly, one cannot derive an unambiguous prediction about the means. Furthermore, we have already noted that the difference between means for the Moderate and Extreme conditions is well within sampling variability (cf. Tables 5.1 and 6.8 for the variances). Perhaps on the basis of the hypotheses one could expect the Extreme condition to be more variable, but such an expectation could not be derived from the hypotheses without additional assumptions. Furthermore, the tables indicate that the variances for the two conditions are similar. Using the conflict model, however, provides a way to test these hypotheses.

In Chapter 4, we identified α as the probability that an individual will reject the group and β as the probability that an individual will change his leanings in the direction of the group. According to the hypotheses, both α and β should be greater in the Extreme condition, which, in fact, is the case. The estimates of these parameters, from the four-parameter model for the Moderate condition and from the three-parameter model for the Extreme condition, are

	Moderate	Extreme
α	.03	.13
β	.12	.39

These estimates are approximate, and since there are no tests of significance, one must be cautious in interpreting this analysis.

The results, however, indicate that parameters could be important and theoretically relevant indices of behavior in this situation.

We suggested earlier that the parameters might be statistics that summarize many important properties of the data. The parameter differences just noted in the previous paragraph, in effect, signify many properties that differ in the two conditions. In the Extreme condition, the amount of conformity is highest at the beginning of the experiment, and then, increasingly, subjects become consistently correct. For practical purposes the process is nearly over at the end of the 36 trials. In the Moderate condition, the process takes place at a much slower rate; it may take more than 36 trials to reach a stable equilibrium, or if it is not really the same process as the Extreme condition, oscillation may be the only equilibrium. If the model is correct, the point of greatest conformity occurs later in the Moderate than in the Extreme condition. It also should be noted that a higher proportion of subjects makes at least one conforming response in the Extreme condition, although the average number of conforming responses is smaller.

We have pointed to only a few of the differences between conditions, and there are many open questions, especially with regard to the Moderate condition. The features we have described, however, are related to our view of the underlying process of conflict resolution. Let us follow briefly one line of speculation concerning the degree of discrepancy, the intensity of conflict and the rate of conflict resolution. First of all, it is clear that the rate of conflict resolution, that is, the rate of movement to the absorbing states, is greater in the Extreme than in the Moderate condition. How can we examine the intensity of the conflict? It may be possible to make use of alternations for this purpose, since we have already noted that this measure may be quite sensitive to changes in the boundary conditions of the experiment.[14] Alternations seem to be appropriate because it is reasonable to assume that the more intense the conflict, the more an individual will switch back and forth. But the mean number of alternations may reflect both the intensity and the duration of the conflict;[15] the probability of an alternation from trial n to trial n + 1 is given by

$$\beta p_2(n) + \gamma p_3(n)$$

hence, this probability depends on the probabilities of being in states 2 and 3 on trial n, or in other words, on the probability of still being in conflict on trial n. Similarly, the mean number of alternations depends on the behavior of $p_2(n)$ and $p_3(n)$. (For given values of β and γ, the faster $p_2(n)$ and $p_3(n)$ go to zero, the smaller the expected number of alternations.) If, however, we ask in which condition is a person who is in conflict more likely to alternate, we can compare the Moderate and Extreme conditions with respect to intensity, and independent of the duration, of conflict.

In the Extreme condition, $\beta = \gamma = .39$, whereas in the Moderate $\beta = .12$ and $\gamma = .21$. But β and γ are the conditional probabilities of an alternation, given states 2 and 3, respectively. In other words, given two individuals, both in state 2 on trial n, the individual subjected to the Extreme condition is more likely to alternate than the individual subjected to the Moderate condition. The same conclusion holds for two individuals in state 3.

Since we view the probability of an alternation as reflecting the intensity of the conflict, we conclude that both the intensity and the rate of conflict resolution vary with the discrepancy between the individual and the group. While we might expect the rate of resolution to vary monotonically with the magnitude of the discrepancy, the intensity of the conflict should begin to diminish when this magnitude becomes sufficiently large. Additional research could enable us to specify further the form of this function and also could uncover other quantities of the model to characterize dimensions of conflict such as intensity.

In addition to being concerned with establishing functional relations between the parameters of the model and the values of boundary variables, we hope to extend the model to apply to more complex experimental situations. For example, if we alter the sequence of trials within the experiment in some systematic way, can we modify the model to describe the new sequence? We turn next to an illustration of the way in which we can make use of our experimental results to generalize our model to a more complex experiment.

One of the criteria of a model's usefulness is its ability to predict new experimental results. Although the present design was based on Asch's Experiment 1, the differences between the design and Asch's are sufficient to consider testing the conflict model on Asch's data a new application of the model. In fact, the present results suggest that we must modify the model before testing it with his data.

Asch combines both Moderate and Extreme trials in the same experimental sequence; in the present work, we have found two distinct sets of parameters for Moderate and Extreme conditions. The conflict model, however, assumes that the transition probabilities are the same on all trials. We can state this assumption in slightly different form: Let a given trial constitute an event; call it E. With this event is associated an operator O which alters the probabilities of being in each state. Each time the event occurs, that is, on each trial, the operator is applied. The form of O is given by the matrix of transition probabilities:

$$O = \begin{bmatrix} 1 & 0 & 0 & 0 \\ \alpha & 1 - \alpha - \beta & \beta & 0 \\ 0 & \gamma & 1 - \gamma - \epsilon & \epsilon \\ 0 & 0 & 0 & 1 \end{bmatrix} \qquad (8.1)$$

and the results of applying this operator are given by the transition equations (Equations 4.6). As long as the environment is constant, it does not matter with which element of the situation we identify E; we could identify E with a trial, with a group response, or with the experimenter turning the stimulus cards. When these conditions do not completely overlap, we must be careful to identify E with that aspect of the environment which produces changes in the subject's behavior. In the present context, we identify E with the occurrence of a unanimous and incorrect response of the role players. The fact that we found differences between sequences of Moderate and Extreme trials suggests that we assume two separate events in order to describe Asch's experiment. Hence: Let

E_1 be the occurrence of a unanimous moderate error on the part of the role players

and

E_2 be the occurrence of a unanimous extreme error on the part of the role players

We then associate the operator O_1 to event E_1 and the operator O_2 to E_2. For O_1 we have a transition matrix like Equation 8.1, with the subscript 1 attached to each parameter. Using the fact that three parameters were sufficient to describe the Extreme condition, we have

$$O_2 = \begin{bmatrix} 1 & 0 & 0 & 0 \\ \alpha_2 & 1 - \alpha_2 - \beta_2 & \beta_2 & 0 \\ 0 & \beta_2 & 1 - \beta_2 - \epsilon_2 & \epsilon_2 \\ 0 & 0 & 0 & 1 \end{bmatrix}$$

After applying these operators, we have the following transition equations:

If E_1 occurs:

$$\left.\begin{aligned}
p_1(n+1) &= p_1(n) + \alpha_1 p_2(n) \\
p_2(n+1) &= (1 - \alpha_1 - \beta_1)p_2(n) + \gamma_1 p_3(n) \\
p_3(n+1) &= (1 - \gamma_1 - \epsilon_1)p_3(n) + \beta_1 p_2(n) \\
p_4(n+1) &= p_4(n) + \epsilon_1 p_3(n)
\end{aligned}\right\} \qquad (8.2)$$

and if E_2 occurs:

$$\left.\begin{aligned}
p_1(n+1) &= p_1(n) + \alpha_2 p_2(n) \\
p_2(n+1) &= (1 - \alpha_2 - \beta_2)p_2(n) + \beta_2 p_3(n) \\
p_3(n+1) &= (1 - \beta_2 - \epsilon_2)p_3(n) + \beta_2 p_2(n) \\
p_4(n+1) &= p_4(n) + \epsilon_2 p_3(n)
\end{aligned}\right\} \qquad (8.3)$$

But for distinguishing these two classes of events, the model is identical with the conflict model discussed in Chapter 4. We shall call this special case the "two-event conflict model."

We should point out some of the features of Asch's Experiment 1 that we are not considering in this model. These were mentioned in Chapter 2, but here we want to make it clear that we assume the following variables have no effect:

1. Interspersing of neutral trials in the sequence.
2. Varying the size of the standard.
3. Varying the relative positions of the comparison lines.
4. Varying the ratio of the discrepancy to the size of the standard.

The only additional factor considered in this model is the variation in the majority response. Furthermore, we assume no interaction between trials on which the majority gives a moderate response and trials on which the majority gives an extreme response. In contrast to the "consistency of group pressure model" of Chapter 3, we consider that the majority response affects the subject's behavior only on the given trial, and we are not concerned with the confederates' change from moderate to extreme as we were in Chapter 3. In other words, we assume that an extreme trial has the same effect whether it is preceded by a moderate, a neutral, or another extreme trial.

For the two-event model we shall not derive any properties, nor shall we estimate the parameters from Asch's data. We shall use the parameter values estimated in the Moderate and Extreme conditions of the present experiment and test the fit of the model by numerical and Monte Carlo techniques.

The values of the parameters are as follows:

$$\left.\begin{array}{ll} \alpha_1 = .03 & \alpha_2 = .13 \\ \beta_1 = .12 & \beta_2 = .39 \\ \gamma_1 = .21 & \epsilon_2 = .015 \\ \epsilon_1 = .02 & \end{array}\right\} \qquad (8.4)$$

Since we have not estimated these values from Asch's data, we are testing parameter values as well as the model.

In Chapter 2 we noted that Asch's experimental sequence was as follows:

N, N, M, M, N, E, M, E, E, N, N, M, M, N, E, M, E, E

where

M is a moderate trial

E is an extreme trial

N is a neutral trial

Since we assume that neutral trials have no effect and that moderate trials constitute E_1, while extreme trials are event E_2, the sequence of events in his experiment is

$$E_1,\ E_1,\ E_2,\ E_1,\ E_2,\ E_2,\ E_1,\ E_1,\ E_2,\ E_1,\ E_2,\ E_2$$

To obtain the theoretical curve of p(n), we substitute the values in Equations 8.4 into the transition equations (Equations 8.2 and 8.3). Then we apply Equations 8.2 or 8.3 to the probabilities of being in S_j from the previous trial. We use Equations 8.2 or 8.3 depending on whether the event on trial n is E_1 or E_2. Using the assumption that all subjects are in S_2 on trial 0, and the fact that event E_1 occurs on trial 1, we have for $p_j(1)$

$p_1(1) = .03$

$p_2(1) = .85$

$p_3(1) = .12$

$p_4(1) = .00$

Adding $p_1(n)$ and $p_2(n)$, we obtain .88 as the desired p(n). In similar fashion we obtain values of p(n) for $n = 1, \cdots, 12$.

The theoretical mean curve is compared with the observed in Figure 8.1. On the whole, the degree of agreement is quite good, especially when one remembers that the parameter values are not optimum for the conditions from which they were estimated. The major discrepancies occur at trials 2, 3, 4, and 10. It is

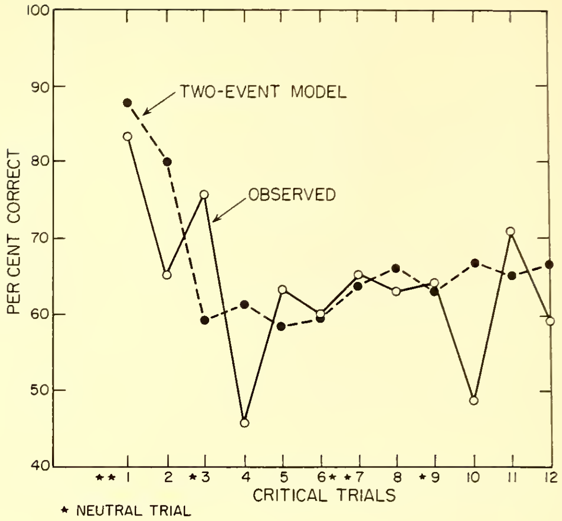


Figure 8.1. Comparison of the theoretical and observed mean curves for Asch's Experiment 1.

$$\text{Two-event model}\begin{cases} \alpha_1 = .03,\ \beta_1 = .12,\ \gamma_1 = .21,\ \epsilon_1 = .02 \\ \alpha_2 = .13,\ \beta_2 = .39,\ \epsilon_2 = .015 \end{cases}$$

interesting to speculate on the question of whether or not the discrepancies at the three later points are caused by the presence of neutral trials immediately before trial 3 and before trial 9.

The next property to consider is the distribution of errors. The model's predictions are obtained by running Monte Carlo subjects, but with two sets of transition rules. The first of these which is applied when E_1 occurs, is the set of rules for the four-parameter conflict model and is found in note 4 of Chapter 7. The rules applied when E_2 occurs are those in Chapter 6 for the three-parameter model for the Extreme condition.

Table 8.1 compares the Monte Carlo and observed distributions. The Monte Carlo data are based on a sample of 50 subjects, whereas the observed sample contains 123 subjects. Thus it is the percentage distributions that should be compared. As the table shows, the model gives rise to a distribution similar to the observed, although the Monte Carlo distribution has no subjects who make 10 or more errors. Accordingly, the Monte Carlo mean and variance are less than the observed. From examination of results of several Monte Carlo samples for the previous models, it seems that these discrepancies are within the range of sampling variation; but this statement must be regarded as tentative because we have examined only a single Monte Carlo sample for the two-event model.

Table 8.1. Comparison of the Distribution of Errors from the Monte Carlo Subjects with That Observed in Asch's Experiment 1

(Two-event model: $\begin{cases} a_1 = .03,\ \beta_1 = .12,\ \gamma_1 = .21,\ \epsilon_1 = .02 \\ a_2 = .13,\ \beta_2 = .39,\ \epsilon_2 = .015) \end{cases}$

Number of Errors	Monte Carlo Subjects (M = 50)		Actual Subjects (M = 123)	
	Mean Frequency	Mean Per Cent	Frequency	Per Cent
0	13	26.0	29	23.6
1	2	4.0	8	6.5
2	5	10.0	10	8.1
3	3	6.0	17	13.8
4	2	4.0	6	4.9
5	8	16.0	7	5.7
6	7	14.0	7	5.7
7	3	6.0	4	3.2
8	6	12.0	13	10.6
9	1	2.0	6	4.9
10	0	0.0	6	4.9
11	0	0.0	4	3.2
12	0	0.0	6	4.9
$\overline{X}$ =	3.8		4.6	
s^2 =	8.79		12.51	

Finally, we should consider the mean and variance of the number of alternations. Table 8.2 presents these statistics for the Monte Carlo and observed samples. Since the conflict model underestimated both the mean and variance in the Moderate condition, we should expect a discrepancy here. But the discrepancy, although in the same direction, is considerably less serious. This finding suggests that the conflict model may be appropriate for describing short sequences, but additional assumptions are necessary to describe longer experimental sequences.

Table 8.2. Mean and Variance of the Number of Alternations from 1 Monte Carlo Sample Compared with Data from Asch's Experiment 1

(Two-event model: $\begin{cases} a_1 = .03,\ \beta_1 = .12,\ \gamma_1 = .21,\ \epsilon_1 = .02 \\ a_2 = .13,\ \beta_2 = .39,\ \epsilon_2 = .015) \end{cases}$

	Monte Carlo Subjects (M = 50)	Actual Subjects (M = 123)
$\overline{X}$	2.6	3.7
s^2	4.0	8.8

Early in the section, we again raised the question of how a mixed sequence of experimental trials would differ from a pure sequence. Clearly, the mixed sequence is considerably more variable than either the Moderate or Extreme condition; hence, one basis for separating these two types of trials is justified. But the fact that we achieve as good a fit as we do with the "two-event" model indicates to us that the same basic process is operating in Asch's more complex experiment. This provides additional support for our belief that in simplifying Asch's work, we retained its essential features. The degree of fit obtained is all the more encouraging in view of the many factors operating in Asch's experiment which the two-event model ignores, and also in view of the fact that we have used no degrees of freedom in fitting this model; that is, no parameters were estimated from the data.

The discrepancies between the "two-event" model and the observations from Asch's Experiment 1 remain to be explained. When they are explained, we shall have a better understanding of how a pure sequence differs from a mixed one. While some of the factors, such as neutral trials, not incorporated in the two-event model undoubtedly will account for part of the discrepancy, it does seem that a mixed sequence produces effects beyond what would be obtained by simply adding the effects of moderate and extreme trials. In other words, we should suggest a modification of this model to allow interaction between these two types of trials in a manner similar to the "consistency of group pressure" model of Chapter 3. Such speculation could be examined best with a new experiment which mixed moderate and extreme trials but which did not contain the other variables of Asch's experiment.

This brief analysis of the two-event model is suggestive rather than conclusive. We must reserve detailed consideration of this case for future investigation. Our results clearly indicate that the conflict model and its generalization to a two-event model should be pursued further, in an effort both to deduce additional properties and also to discover new empirical applications.

8.4 Extending the Conflict Model

The illustrations of the last section in themselves suggest many paths for future research. Furthermore, throughout the course of the monograph, we have alluded to possible new experiments that arise from using the conflict model as a heuristic tool. Moreover, we have discussed alternative formulations that would generate new models. In this final section, we shall present a few examples of studies now being planned to illustrate further the fertility of the conflict model.

Before considering these studies, we should mention one concern that remains with us, that is, the development of better methods for estimating the parameters of the model. Mathematicians who have examined the model have suggested that it should be possible to derive expressions for other properties of the situation, such as the distribution of run lengths, which would enable us to solve four independent equations for the parameter values.[16] We are presently attacking the estimation problem with the objective of improving our analytic tools and refining our computer procedures.[17]

In the previous section, we analyzed the effect on the parameters of the model of changing a boundary condition of the experiment. We now want to ask a more general question; namely, what aspects of the underlying process change as a result of changing a specific condition of the situation? We have related the parameters to two different modes of the confederates' responses, or as we interpret this boundary variable, two different levels of social pressure. But modifying other features of the situation may require us to alter elements of the model in addition to the values of the parameters. Or, changing some of the assumptions of the model may require us to construct new experiments that depart from the Asch situation. Of these two approaches, we have adopted the latter; our procedure is to use the model heuristically, to inquire about the consequences of changing our assumptions, and the procedures to test these consequences.

The assumption that all subjects are intially in state 2 is our point of departure for generalizing the model. In Chapter 4,[18] we indicated some lines of further experimentation. The interpretation that all persons intially "leaned away from the group" rested on the presence of an internal standard built up through previous experience. In that chapter we suggested that it would be fruitful to search for situations where the initial distribution of subjects would differ from that postulated for the present experiment. Here we want to describe two specific experiments where the model, with appropriate modification of this assumption, should apply. The first involves the postulate that half of the subjects are in S_2 and half in S_3 at the start of the experiment. According to the interpretation in Chapter 4, this postulate should hold when there were no internal standards and two alternative external norms. (It would also hold when there were two internal standards of "equal strength," but this would be difficult to create in an experimental situation.) We belive that a modification of the autokinetic experiments will produce the state of affairs our assumption describes. Instead of judging the distance moved by the pinpoint of light, the task will be to judge the direction of movement, i.e., right or left, up or down. With the naive subject there will be two groups of confederates, one of which will give one alternative response, e.g., "up," while the second will give the opposite response, e.g., "down." Of course, we shall require an equal number of role players in each group. We plan a sequence of experimental trials that could be analyzed in the same way as the present experiments. Working out the details of this experiment, however, is not trivial; equating the social pressure applied by the two groups of confederates and controlling the order of the confederates' responses are only two of the problems. If we are successful in carrying out this experiment and in analyzing it in terms of a four-state model, our success would indicate greater generality for the approach we have taken.

In Chapter 4, we also noted the difficulty of designing a nontrivial experiment where subjects all began in state 3. By nontrivial we mean a situation where the probability of moving from S_3 to S_2 was greater than zero. Including a "payoff" in the situation similar to that used by Siegel[19] seems appropriate for creating the desired conditions. In this experiment, we shall use ambiguous stimuli with two response alternatives and have a single unanimous group of confederates. The confederates will respond consistently, choosing the same alternatives at each trial. The experimenter will announce that there will be a monetary reward for the "correct" response; the sum of this reward will be fixed, and we shall make it clear to the subject that the reward will be divided equally among those members of the group who are "correct." If our view is correct, in this experiment there should be an initial predisposition to conform to the group, but also some pressure to "go it alone." The design of this experiment is at present in a beginning stage; nevertheless, our preliminary conception does illustrate another direction for extending our model.

Perhaps the most intriguing research questions now being considered are those directed at discovering additional overt behaviors that can be tied to the underlying process. It is in this area that the model has great potential utility. If we can define a set of rules relating overt behavioral processes to the underlying states, we should be able to interrelate these manifest processes. This line of investigation can take us far afield from the Asch situation; indeed, we believe this type of model will apply to a number of two-choice decision-making experiments.

In the Asch situation itself, there are other observations that we plan to collect and to relate to the underlying states. Our hypothetical process involves passage from conflict states to resolution states and describes the consistency of conforming (nonconforming) responses in these terms. But there are other indicators of conflict. Such things as nervous manifestations, hesitancy in responding (latency of response), and confidence in one's answer may all be related to the subject's state on trial

n. If such a relationship could be formulated, then, for example, we chould show that when the subject becomes consistent, he also gives his responses more rapidly, or that when he becomes consistent, his signs of nervousness disappear.

In addition to seeking further indicators in the Asch experiment, we plan a new study that does not deal with conformity but rather with two-choice decision making where there are no social pressures operating. If we can create a situation where the subject has no predisposition to choose either of the two alternatives available to him and does not acquire any basis for his choice as the sequence progresses, we believe a special case of our model will apply. Since the subject will have no basis for his choices, we believe that he will never become consistent. That is, we anticipate that a model with zero transition probabilities to the absorbing states will describe this experiment.

We hope that the new experiments and alternative formulations presented in this chapter are a convincing demonstration of the fertility of the conflict model. Some of these will be discarded, but some will enable us to supplant present conceptions. As we have indicated, there are many unsolved problems both in the present model and the extensions we have suggested. But generating such problems is one function of model building. We believe that efforts to solve these problems together with pursuit of the studies we have discussed will open up paths toward more cumulative understanding of conformity. There is much work to be done, but if we are correct in our belief, then the conflict model will play an important part in the formulation of rigorous, empirically supported theories of conformity.

NOTES

1. See Chapter 1, pp. 9 ff.
2. For example, Asch was concerned with the issue of temporal trends, lest summing individual scores over trials obscure an important property of the experiment.
3. The "individual differences model," see pp. 36-41.
4. It should be emphasized that the baseline models played an important role in this investigation and can play such a role generally. But they are a stage, not an end point in the research process.
5. The more abstract conceptualization continues to serve heuristically. In considering extensions of the model, it enables us to raise questions that in themselves provide direction for future work. We shall return to this point later.

6. There are other lines along which to explicate the concept of conflict. For example, if withdrawal from the field were a possible mode of resolution, in addition to the two modes of resolution we have discussed, then we might want to postulate a fifth state, that of withdrawal or escape. In the Asch experiment, however, subjects were forced to respond on every trial, so that withdrawal as an observable mode of resolution was not present. Even though there are no additional behaviors to tie to this state, it might be fruitful to assume such a subjective state. The problem would then be to establish rules by which the conforming and nonconforming responses are related to presence in the withdrawal state.

7. It is conceivable that one might want to allow for sudden, one-trial resolutions at the opposite pole of the conflict, particularly in situations where there were long intervals between trials or where other tasks were interpolated between successive judgments. But how we would handle the complications introduced by making the conceptualization more general is not clear at present.

8. The implications of this distinction were only dimly recognize until now. Thus, in Chapter 4, the definitions given for the state are not definitions but rather rules by which to relate the underlying conflict process to overt conformity behavior. The states remain to be defined in a rigorous fashion that when accomplished should further explicate the notion of conflict.

9. This assumption could be relaxed somewhat without increasing theoretical or practical difficulties if there were some *a priori* way to partition the sample into classes. Such a partition would be equivalent to asserting that the transition probabilities are functions of some variable characteristic of the subjects. Since the number of subjects in each class would have to be substantial in order to provide stable estimates of the parameters, such a partition was not feasible with the Moderate and Extreme conditions. We had hoped to examine such a partition with Experiment 3, but the lack of correlation between conformity responses and our personality measures indicated that such an investigation would not be fruitful for these particular tests.

10. An alternative model, for example, should assume that trial 0 was the first experimental trial and that $p_1(0) = 0$; $p_2(0) =$ the proportion of correct responses on the first experimental trial; $p_3(0) =$ the proportion of conforming responses on the first experimental trial; $p_4(0) = 0$. This model is presently being investigated. (For the rationale behind assuming that no one is initially in an absorbing state, see Chapter 4.)

11. It is interesting to note that the assumption of constant transition probabilities did not create the same difficulties in the analysis of the Extreme condition. Although the stimulus sequence in both conditions was the same, the variation in the side where the standard was presented did not produce serious discrepancies between the model and the data in this condition. This suggests that the effect of position of the standard may be an interaction effect and leads us to the speculation that the greater discrepancy between the line the individual perceives as correct and the line the confederates choose as their response, the less the effect of other properties of the stimulus series.

12. See especially notes 6, 8, 9, and 10 in this chapter.

13. In Section 8.4, we shall mention several other boundary variables that seem particularly interesting.

14. See Chapter 5.

15. By duration, we mean the number of trials before absorption.

16. Several approaches to deriving new properties of the model are under investigation. We are indebted to J. L. Snell for a procedure that uses part of each subject's sequence of responses before he becomes consistent to obtain maximum-likelihood estimates of the parameters. As long as the subject continues to alternate, we have complete information tying his response to a state of the underlying process. Snell's procedure uses this information to describe the entire sequence including the responses after absorption. There is, however, some question about the utility of this method. The issue is whether or not complete information about part of the data is sufficient, particularly in view of the high proportion of subjects who do not alternate at all.

17. Another approach to obtaining additional expressions to solve for the parameter values involves an unusual application of a computer. That is, we intend to program the computer to write equations for the estimation of parameters. Earlier, we derived points Π_k of the distribution of the errors by examining subsequences of the forms 2---2 or 23 and noting that the probability of the former is $1/(\alpha + \beta)$ and the probability of the latter is β. The probability of any given sequence was formed by taking the product of the probabilities of the component subsequences; the probability of exactly k errors was then found by taking the sum of the probabilities of all sequences leading to exactly k errors. In the earlier analysis, we argued that it would be impractical to proceed with deriving each point of this function because the number of differing sequences

involved soon became cumbersome. The computer, however, eliminates this difficulty. It is possible to program the computer to generate all the sequences leading to precisely k errors and then to examine each sequence for its components. Using numerical symbols for the probabilities, the computer can then form the required products. Output would be these products whose sums could easily be formed. It would be necessary to simplify these algebraic expressions to obtain equations for estimating the parameters, and this step could pose the most serious problem. It is hoped, but by no means guaranteed, that examination of the expressions for the points of the distribution function would lead to results in simple form so that we could solve a system of simultaneous equations.

18. See pp. 60 ff.

19. S. Siegel, personal communication with the author.

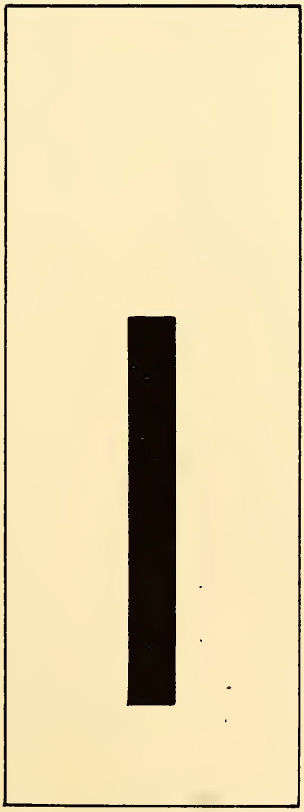
APPENDIX A

A.1 Instructions to Subjects

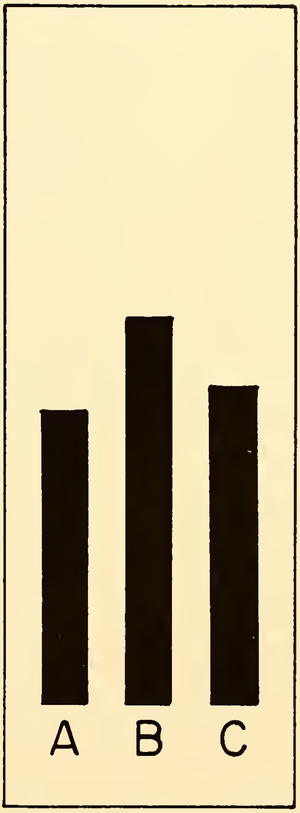
This is an experiment in visual perception. It is a task involving the discrimination of lengths of lines. I shall show a pair of cards. On one of these will be a single line; on the other will be three lines differing in length. Each of the three lines will have a letter beneath it to identify it. One of the three lines will be exactly equal to the single line on the other card — you will decide in each case which is the equal line. You will state your judgment in terms of the letter identifying the line. Sometimes the card with the single line will be on your right, and sometimes it will be on your left.

As the number of comparisons is few and the group small, I shall call upon each of you in turn to announce your judgments, which I shall record here on a prepared form. Please be as accurate as possible. Suppose you give me your estimates in order starting at the left. Are there any questions?

A.2 Examples of Experimental Stimuli



Standard



Comparison lines

APPENDIX B

B.1 Experimental Data

Moderate Condition

Subject	Sequence of Responses (1 = correct, 0 = incorrect)
202	111111111111111111101111111111111111
203	100000100000000000000100000000000000
205	111100111000111101111111111111111111
206	110010000100000000100000000000000000
207	111111111111111111111111111111111111
208	110111010000000000000000000000000000
209	111111111111111111111111111111111111
210	100100000000000010000000000000000000
211	111111111111111110111111111111111111
212	111111101110111111111111111111111111
213	111101111111111111111111111111111111
214	101101011010101111101000010101110100
215	111111111110111111111110111111111111
216	111111111111111111111111111111111111
217	110101110111100110000001101000000000
219	110011011111101000110110110001110111
220	101101101110101110101111110101111011
221	111111100000111000100000000000000000
222	111111101111011011111111111111111111
223	101111001010001011111000001101011111
224	111111111111111111111111111111111111
225	111111111111111111111111111111111111
226	111101010100011000010010001000001000
227	111110011011011001110110001010000000
228	111111111111111111111111111111111111
229	101111101011101110101100010001111000
230	110000000000000000000000000000000000
231	111111111111111111111111111111111111
232	111111111111111111111111111111111111
233	101011100111100010110000010101100001
234	111111111111111111111111111111111111
235	111111111111111111111111111111111111
236	110011001111011101011110111111110111

Extreme Condition

Subject	Sequence of Responses (1 = correct, 0 = incorrect)
301	100000010000000000000101010000000000
302	111111111111110110010001011111111111
303	111001100011111111111111111111110111
304	111110111011111111111111110111111111
305	111111111111111111111111111111111111
306	101101110110100011011111111111111111
307	111011111111111111111111111111111111
308	001100101111101101111111111111111111
309	111111111111111111111111111111111111
310	100101111111111111111111111111111111
311	101100000010000011010001100100110111
312	111111111111111111111111111111111111
313	101100111111101111111111111111111111
314	110110111111111111111111111111111111
315	111111111111111111111111111111111111
316	111011001010111001111111110111111111
317	101000001010101000000000000000000000
318	101000000000000000000000000000000000
320	111111111111111101111110111111111111
321	111111111111111111111111101111111111
323	100000000000100000000001000000000110
324	111111111111111111111111111111111111
325	111111111111111111101111111111111111
326	111111111111111111111111111111111111
327	001111001111111111111111111111111111
328	111110111111111111111111111111111111
330	001111111111111111111111111111111111

B.2 Analysis of Special Devices

Table B.1. Effect of Varying the Side of the Standard with Respect to the Comparison Lines

	Moderate (M = 33)		Extreme (M = 27)	
	Standard to the Right	Standard to the Left	Standard to the Right	Standard to the Left
Number of Errors	163	192	96	110
Per Cent of Possible Errors	27.4	32.3	19.8	22.6
$\sum$ of Ranks* of Less-Frequent Sign	237.5	——	125.5	——
z	.76		1.56	
p	.60		.24	

* Wilcoxon Signed Rank Test. See Walker and Lev (1953).

Table B.2. Effect of Varying Stimulus Labels

Stimulus Label:	ABC	DEF	GHI	JKL	MNO	PQR	STU
Moderate (M = 33)							
$\sum$ Ranks*	131.5	123.0	146.0	132.5	129.5	139.0	122.5
Mean Rank	3.98	3.73	4.42	4.01	3.92	4.21	3.71
$\chi^2_R = 2.32$ $p > .75$							
Extreme (M = 27)							
$\sum$ Ranks	112.0	109.5	117.0	97.0	110.5	117.5	92.5
Mean Rank	4.15	4.05	4.33	3.59	4.09	4.35	3.42
$\chi^2_R = 4.71$ $p > .50$							

* Friedman's Analysis of Variance for Ranked Data. See Walker and Lev (1953).

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